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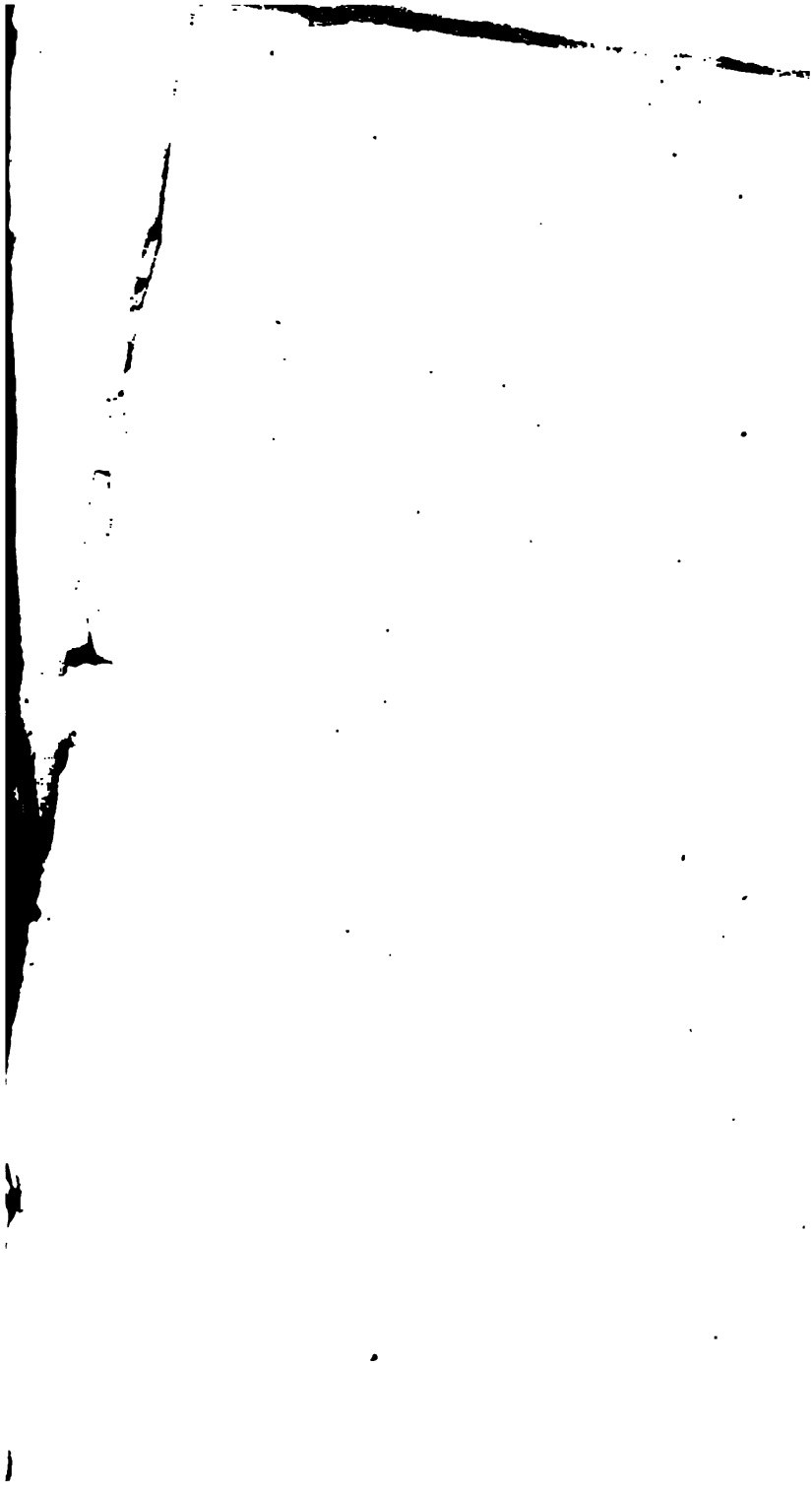

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A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

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THE

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1. JANUARY 1814.

PART I.

ORIGINAL COMMUNICATIONS.

I.

Sketch of the Life and Writings of the late Mr BENJAMIN GIBSON, Surgeon in Manchester. By JAMES WARDROP, F. R. S. Edinburgh.

BIOGRAPHY has ever been considered as an interesting and useful branch of knowledge. The powerful influence of example on the human mind must have impressed man ever since the rudest ages, with a desire to perpetuate the glorious deeds of war; and, in proportion as those, who devoted themselves to the cultivation of the mind and the investigation of science, have gained rank and importance in society, in the same degree have their lives and characters claimed the attention of future ages.

The biography of eminent men in the various departments of civilized life, has thus become one of the richest fountains of human knowledge; it creates an interest which a mere detail of facts and a relation of opinions can never excite, whilst at the same time it calls forth a laudable feeling of ambition and honourable distinction, which cannot fail to produce the most beneficial effects on individuals, and have a happy influence on the welfare of the community.

Though the sphere of life in which the medical man revolves, cannot excite the public interest, nor his professional fame rival that of the warrior or patriot in the admiration of posterity, yet the duties imposed on him as a member of society, are so much

intermingled and so closely linked with the happiness and prosperity of mankind, that he who makes use of his best efforts and an honourable zeal in cultivating a liberal profession, and employs the energies of his mind in devising means to alleviate the distresses of his fellow creatures, has a just claim on public gratitude and esteem.

Impressed with these sentiments, I have thought that some account of the life and writings of the late Mr Gibson might not be unacceptable to the medical public, not only as offering a small tribute of respect to the memory of an individual, who must have been regarded as one of the brightest ornaments of his profession; but also as affording an opportunity of giving a sketch of his valuable writings, and of the interesting improvements he made in different branches of surgery.

Mr Gibson was born in September 1774, at Newcastle upon Tyne, of most respectable parents, whose situation in life afforded him all the usual means of obtaining a liberal education. From the age of eight to that of seventeen, he remained at an excellent school kept by Mr Temple, at Richmond, in the north Riding of Yorkshire. Having there laid a good foundation of classical and general literature, he began his medical pursuits by serving an apprenticeship of five years to Mr Ingham, a surgeon of Newcastle, whose celebrity was not confined to that town and neighbourhood, but extended over a wide circle of the surrounding country; and who still enjoys the same honourable rank in his profession, though prevented by advanced years and impaired health from undergoing its more fatiguing duties. In this situation, Mr Gibson had an opportunity of learning the rudiments of his profession, and acquiring that facility of discriminating diseases, and readiness in the application of the curative means employed in medicine, which seem only to be easily and eminently obtained by practice in early life, a facility which those more advanced in years can never acquire but with unwearied diligence and labour.

In the year 1796 he went to London, chiefly with a view to prosecute his anatomical studies under Dr Baillie, at that time the most eminent teacher in the metropolis. It was in this department of his pursuits that he first made a conspicuous figure amongst his contemporaries. From early youth he had evinced a very unusual mechanical talent, and possessed an extreme nicety in mechanical execution. These, with a most persevering and well-directed assiduity, were applied to anatomical researches, and it may be readily conceived, that thus gifted, the dissections which he performed, and the numerous preparations which he made, possessed a neatness, and displayed an anatomical skill

almost

almost unrivalled. Many lasting monuments of this talent now remain, particularly a series of preparations exhibiting the structure of the ear in the museum of the University of Edinburgh, and an extensive private collection at Manchester, chiefly intended to illustrate the anatomy of the organs of sense. Such indeed was his dexterity, that, when a young student, he amused himself with cutting a cherry-stone into the form of a human cranium, and detailing, in that minute scale, with incredible accuracy, the various eminences and depressions of the bones.

After spending two winters in London, he visited Edinburgh in the year 1798. There he passed the winter-session and part of the ensuing summer, attending the most celebrated lecturers of that university, and displaying his usual zeal and industry as a student. In consequence of the very favourable opinion which had been formed of his abilities, he was solicited in the autumn of 1799 to assist Mr White of Manchester, who was at that time about to withdraw himself, in part, from the fatigues of business *.

After having been actively engaged in the practical part of his profession, for nearly eight years, along with Mr White, Mr Gibson began his own professional career.

He had already acquired no small degree of medical fame, and public confidence; and, besides an extensive private practice, he was appointed one of the surgeons of the Manchester Infirmary, an institution where perhaps there is a greater field for surgical experience than any in this island, those of the metropolis not excepted. He had, therefore, every opportunity of cultivating his profession, and these he embraced with eagerness and prosecuted with assiduity.

In every thing that regarded the operative part of surgery, he pre-eminently excelled, having great ingenuity in devising means, and possessing a degree of nicety and dexterity in execution which few have ever acquired. But he had still more important qualifications.

The native powers of his mind, improved by early application and habit, had given him a great facility in distinguishing diseases, and of perceiving all those delicate shades of difference, that escape the eye of a common observer. This faculty is, perhaps, the most essential to the medical practitioner; for on it depends

* It might be well to state, that Mr White had applied to the professor of anatomy in Edinburgh, and to some of the principal anatomical teachers in London, to recommend the most eligible person of their acquaintance for so important a trust, and, that without any communication with each other, they all concurred in the selection of Mr Gibson.

pends the great art of adapting particular remedies to particular diseases, and of modifying their exhibition according to the peculiarities which these diseases assume in every individual.

Besides, his anatomical knowledge, and his familiarity with the examination of morbid appearances, made his acquaintance with diseases, and his knowledge of the means necessary for their treatment, of the most accurate and comprehensive kind.

His unusual dexterity in the operations of surgery, and his long habits of delicate and minute dissection, particularly of the organs of sense, led him, from a very early period of his practice, to pay great attention to the diseases of the eye. Not only did he become a most dexterous operator on that organ, but some of the operations he materially improved, and he bestowed great pains on the investigation of the treatment of all its diseases.

There were no features in Mr Gibson's mind more to be admired, than his eagerness in acquiring, and his zeal in diffusing knowledge. In the year after his first settlement in Manchester, he began to give lectures in anatomy, which were attended by all the medical students at Manchester; and he took great pains to communicate to his pupils the results of his private, as well as his public practice. As a lecturer, (I have been informed by one who was a frequent attendant,) he evinced a degree of talent, which, had circumstances led him to the metropolis, or to any other suitable field for its exertion, would have raised him to the highest distinction. An appearance extremely prepossessing; a voice, though somewhat feeble, yet agreeable in its tones, and distinct in its utterance; a lively interest in his subject, expressed by his animated countenance and manner; and, above all, the perfect knowledge of his science, and the neatness and lucid order of his demonstrations, singularly fitted him for becoming a popular teacher of anatomy, and for disputing the palm with those who have been most eminent in that honourable profession. The same mode of thinking which led him freely to communicate the improvements which were constantly suggested by his fertile and industrious mind, created feelings of contemptuous indignation towards those who attempted to conceal any professional discovery, either with the view to add unmerited lustre to their name, or for the mere accumulation of gain. These feelings had become, towards the close of life, unusually acute; for, at a time when his mind had become irritable, from the ravages of the disease which ultimately proved fatal, some important practical improvements of his own had been brought forward as the discoveries of others, and artfully announced to the world as secret remedies.

It

It would be out of place for me to attempt to dwell on his private virtues. These will long linger in the remembrance of that circle of society who had the good fortune to enjoy his friendship; and the number of individuals is not small, who retain a grateful impression of his professional aid and benevolent disposition.

Mr Gibson being naturally of a delicate fabric, the ardour of his mind surpassed the strength of his frame; and, at the same time, the anxious and laborious duties of his profession contributed no doubt to shorten the period of his existence. He had for a long time shown a great predisposition to pulmonary complaints; and at last he was seized with symptoms of confirmed phthisis, the slow progress of which warned him of his approaching fate, whilst they were not so severe as to prevent him prosecuting his professional pursuits, till a few days before his death. He died on the third of February 1812, having just reached his 37th year.

In his writings, Mr Gibson has left some valuable memorials of his industry and talents. The first of these was an Essay read in April 1801 before the Philosophical Society of Manchester, and published in the 1st volume of the 2d Series of their Memoirs, and is entitled, "*Observations on the Effects of Madder-Root on the Bones of Animals.*"

This paper leads to establish a very important physiological fact, not only with regard to the formation of bone, but it refutes some doctrines which had been built on an erroneous explanation of the mode by which madder changes the colour of the bones of living animals.

It had been sometime known to physiologists, that the bones of a young animal, fed on madder-root, acquired a red colour; but Dr Rutherford was the first person who gave any thing like a satisfactory explanation, why the colouring-matter was communicated to the bones alone, of all the white parts of the body. He conceived that the colouring-matter of the madder had a superior attraction to the phosphate of lime, that it united with that part of it which was floating in the general mass of blood, so that all future bone which was formed had a red colour. From the manner in which the coloured portion is deposited, Duhamel's opinion of the lamellated structure of bone was supposed to receive an additional confirmation; and the alternate appearance of red and white laminæ, produced by feeding the animal with, or depriving it of the madder, were considered as convincing proofs, that the harder parts of the animal frame were constantly undergoing the same waste and repair, which had been generally believed to take place in the softer parts. Mr Gibson,

after

after pointing out the erroneous conclusions to which this doctrine leads, explains the phenomena in a very ingenious and much more satisfactory manner. He found, by experiments, that the serum of the blood had a much greater attraction for the madder than the phosphate of lime, so that when an animal has madder mixed with its food, the blood becomes changed with it, and the superabundant colouring matter is imparted to the phosphate of lime contained in the bones already formed. But as soon as the animal has ceased to receive the madder, and the blood is freed from the colouring matter by the excretions, the serum then exerts its superior attraction, and by degrees entirely abstracts it from the phosphate of lime, and the bones resume their natural whiteness. In short, the bones are at one time dyed by the colouring matter, and at another bleached by the serum.

In December 1803, Mr Gibson read another before the Manchester Society;—a paper "on the Use of the Sutures in the Bones of Animals." He wished to prove, that the existence of sutures was entirely subservient to the process of ossification, and that any advantages which may generally have been supposed to be derived from their presence in arresting the progress of a fracture, or in aiding the delivery of the child, were but of secondary importance. The bones of the body are generally increased in their length or extent, not by any uniform extension of their whole substance, but by an addition of bony matter in some particular part. Thus, in the long bones, the process of ossification extends from the middle towards the extremity of the bone,—whilst in the flat bones of the cranium, this process begins at a certain number of determinate points, and the ossification extends from these in all directions, until a complete encasement of bone is formed. It is by the presence of sutures in the cranium, that this mode of ossification can be carried on, and whenever the process is completed, the sutures, being no longer necessary, disappear.

Two original essays of Mr Gibson's were communicated to this Journal. The first of these, "On the Common Cause of the Puriform Ophthalmia of New-Born Children," was published in the 1807, in Volume III. p. 159.

The chief object of this paper was to point out the important observation, that the puriform ophthalmia of children is a disease which the child receives during delivery, while passing through the vagina of the mother affected with fluor albus. He is thus enabled to suggest the probable means of preventing the child from receiving the infection; and the fact itself bears a striking analogy with what is well established regarding inflammations of all

all other mucous surfaces, the puriform ophthalmia of adults and gonnorrhœa being also propagated only by the actual contact of the diseased matter.

Till this time, Mr Gibson had only become known to the world, from the contributions which he had made to the Transactions of Societies or to public Journals. But a short period before his death, he published a separate work, which established, on the firmest foundation, the high opinion which had been formed of his professional talents.

The work is entitled, "Practical Observations on the formation of an Artificial Pupil in several deranged States of the Eye; to which are annexed, remarks on the Extraction of the Soft-Cataract, and those of the Membranous kind, through a puncture in the Cornea Illustrated by plates. London, 1811."

This work is divided into four sections.—The first contains the history of the operation for forming an artificial pupil.—The second is on the mode of forming an artificial pupil, adapted to different deranged states of the eye.—The third, on a mode of extracting a soft cataract through a puncture in the cornea;—and the fourth, on the extraction of several kinds of membranous cataracts.

In the first section is contained an account of the various modes which have at different times been either suggested or employed for making an artificial pupil. Of the defects to which all these modes of operations are liable, every one who has attempted to perform them must be well aware; and such has been the discredit into which they have all fallen, that they have of late years been almost entirely abandoned.

The numerous cases where an artificial pupil appeared practicable, rendered any successful mode of performing the operation one of the great desiderata of surgery, and in accomplishing this desirable object, Mr Gibson advanced a very important step.

In the second section, Mr Gibson entered into a full and detailed account of his new mode of operating; of the different states of the eye which admitted relief; and of the kind of operation adapted for the different cases.

It would be foreign to the object of the present paper, to enter into a minute discussion of all these interesting topics. Every part of this section of the work merits the attention of him who is to perform such operations. The following extracts may perhaps give a general idea of the principles of his mode of treatment. "In describing the following operations," says the ingenious author, "I shall select a case in which the transparent part of the cornea is situated towards the outer angle of the eye. I shall

shall suppose also, that its extent is one-third of the diameter of the cornea, although an opacity leaving a much larger transparent surface may destroy what may be termed useful vision.

"The eyelids being secured, a puncture is to be made in the cornea with a broad cornea knife, within a line of the sclerotica, to the extent of about three lines. All pressure is now to be removed from the eyeball, and the cornea-knife gradually withdrawn. The consequence of this is, that a portion of the aqueous humour escapes, and the iris falls into contact with the opening in the cornea, closing it like a valve. A slight pressure must now be made upon the superior and nasal part of the eyelid, with the fore and middle finger of the left-hand, till at length, by an occasional and gentle increase of pressure, or by varying its direction, the iris will gradually protrude, so as to present a bag of the size of a large pin's head. This protruded portion is to be cut off with a pair of fine curved scissars, and all the pressure at the same time removed. The iris will then recede within the eye, and the portion of it which has been removed will leave an artificial pupil, more or less circular."

After a minute description of the operation, as adapted to different sets of cases, Mr Gibson concludes with observing, "That the permanency of the artificial pupil, appears to me to depend principally upon the size of the opening and healthy state of the iris and contiguous parts of the eye, at the time of the operation. When the artificial pupil has been made almost as large as the medium size of the natural one, and especially when the part of the iris removed has included the border, I have never seen any disposition of the opening to close. When, however, a more narrow slip has been removed, when the iris, from previous inflammation, has become more vascular than natural, or when it is complicated with adhesions to the crystalline lens, in such cases its closure has occasionally taken place. The degree of vision restored by an operation of this kind, is not quite so perfect as that which succeeds the removal of a cataract. In most cases, however, when the cornea has been perfectly transparent to the full extent of one-third of its diameter, the patient has been able to read tolerably small print."

"I have paid considerable attention to the state of the artificial pupil after the eye was quite recovered, in order to ascertain whether the fibres of the iris possess any power of motion, so as to alter the size of the new opening, but have never been able to detect any thing similar to the contraction and dilatation of the natural pupil. It appears to me, that although the new pupil may sometimes close when the iris is in a morbid state, yet, that this membrane, a few days after an operation, has very little

little disposition to contract, in whatever situation the artificial pupil may have been made."

After detailing the various states of the eye where the formation of an artificial pupil may be successfully attempted, and illustrating the whole with very satisfactory cases of his new method of operating, Mr Gibson has added many judicious and candid remarks on those derangements of the eye, where the operation was fruitless, and which leave little room for the assistance of art.

Upon the whole, the mode of making an artificial pupil, proposed by Mr Gibson, must be considered as possessing many important advantages over all former methods of operating. And in some cases where I have had an opportunity of performing it, the result has been extremely successful. The degree of success, however, from the nature of the cases which require such an operation, must be very various; and though in some favourable instances, it may be undertaken with the hope of very essential benefit, there are many others where the relief to be obtained will be extremely doubtful.

In the *third section*, Mr Gibson describes a new method of operating in those cases of soft cataract, where, from the age of the patient, the flatness of the cornea, or the hollowness of the orbit, the more eligible and complete operation of *extraction* cannot be advantageously employed. In such cases, Mr Gibson advised that a couching-needle be introduced in the usual manner, and that it should be the aim of the operator to rupture most freely the anterior part of the capsule, and to break down the substance of the cataract, by passing the couching-needle through it in different directions, that the aqueous humour may more readily act on it, and reduce it to a pulpy state. When this has been effected, and the eye has perfectly recovered from the operation, the surface of the cataract will probably have put on a flocculent appearance, and be fit for extraction.

When the cataract is in this state, instead of repeating the operation of couching, or of having recourse to the usual mode of extraction, by making an *incision* of the cornea, Mr G. proposes, that merely a *puncture* be made in that tunic, through which the portions of the diseased lens are to be removed.

"In performing this operation, the patient is to be seated and secured in the same manner as for the extraction of a cataract. The cornea-knife, of the largest size, is then to be introduced through the cornea, towards the outer angle of the eye, at the usual distance from the sclerotic coat. If there be any doubt of the free laceration of the anterior part of the capsule of the lens, the point of the cornea-knife should be directed obliquely

through

through the pupil, so as to make a more free division of it. All pressure on the eye-ball must now be avoided, and the cornea-knife gradually withdrawn, which is attended with the evacuation of the aqueous humour, and some portion of the cataract. The curette is next to be introduced through the incision, and advanced towards the pupil, by which the whole of the cataract may commonly be by degrees removed in a pulpy state, so as to render the pupil perfectly clear. Its removal is generally much facilitated by gentle pressure towards the vitreous humour with the convex surface of the curette, whilst the point is inserted through the pupil. Sometimes, however, the cataract is not reduced to a sufficient degree of softness by the action of the aqueous humour, and this state makes its removal more low, but seldom renders the repetition of the operation necessary. For when a considerable portion of the cataract has been removed, the remainder is generally observed to be so much reduced in bulk before the fit period for another operation, as to ensure its speedy disappearance."

Mr G. afterwards points out the changes which take place in the appearance of the lens, that render this mode of operating adviseable, and thus concludes: "From much experience, therefore, of the puncture of the cornea, I have found it to be an operation productive of the slightest possible injury to the eye; and I would recommend its adoption in extracting the soft cataract, after the couching-needle has been employed without success, upon these grounds, that it generally accomplishes at once, what might require the introduction of the couching-needle several times, and that it is attended with less risk and irritation to the eye, and gives the patient less pain.

The *fourth section* treats of the extraction of membranous cataracts through a puncture in the cornea. "The variety, (says Mr Gibson,) which I shall select for explaining the mode of operation, is a simple membranous cataract, which has not contracted adhesions with the iris, but has either existed from birth, or has remained after the extraction or depression of the lenticular cataract."

In such a case, the point of the cornea-knife, after penetrating the cornea, is to form a small puncture in the membranous cataract, as near as possible to the margin of the iris, towards the external angle of the eye. The knife is then to be quietly withdrawn, and, by the escape of a part of the aqueous humour, the pupil becomes dilated by pressure *a tergo*, and the pupil in the membranous cataract, is sometimes a little enlarged. Through the puncture the small hook is to be passed behind the opake membrane, with its point directed downwards until it reaches the

the opposite part of the membranous cataract, next the internal angle of the eye. The point of the hook is now to be directed forwards, and is to be passed through the membrane so as to lay hold of it. By gently drawing with slight extracting efforts towards the opening in the cornea, the whole or a considerable part of the opake membrane may generally be removed. Vid. page 128.

The great advantages from Mr G.'s mode of operating, both in soft and membranous cataracts, arise from the smallness of the wound of the cornea, and the little inflammation which seems to ensue.

The facts which he has brought forward, and the reasoning employed, are sufficient to recommend the practice to other surgeons.

A few months before his death, Mr Gibson published in this Journal, Vol. VII. p. 394, a paper entitled, "On the Use of the Couching-Needle in Infants of a few months old." The records of the most respectable authorities had long proved, that the operation of couching might be safely and successfully performed on young children. But the practice of operating on infants was by no means generally adopted, and those born with cataracts were commonly considered as unfit subjects for an operation, until they had arrived at that period of life, when, aware of its importance, they could of themselves willingly submit to have it performed. Mr Gibson, therefore, had the merit of banishing from the minds of surgeons the imaginary difficulties and disadvantages which had been conceived to attend the operation on infants, and of establishing, by a long and extensive experience, an easy and successful mode of performing it.

Towards the close of life, those upright and liberal principles which had guided Mr Gibson throughout his professional career, became the source of great anxiety and irritation, when the actions of others, whose conduct appeared to him exceptionable, came more immediately under his observation. He had himself toiled more for an honourable and lasting fame, than for that pecuniary reward which is too apt to influence the medical practitioner. The present paper, was evidently intended to lay open to the public an operation which he conceived was attempted to be kept secret, and claimed as the invention of another. At the same time, he wished to describe an operation which he himself had been in the habit of performing for a considerable number of years. Since this paper appeared, the posthumous works of the late Mr Saunders have been published, and the history of a mode of operation for cataract, in some respects similar to that

that of Mr Gibson's, is also recommended to be performed in infancy. Thus, by comparing these two publications, the impartial reader is enabled to form a just estimate of the feelings and principles which conducted each of these surgeons, and of the claims of each to any originality or priority of invention.

Charles Street, St James's,
October 1813.

II.

Case of Concussion. By Dr Thomson, of Halifax.

Wednesday, February 3d, 1813.—John Firth, aged 21, whilst tightening a rope on a loaded cart of hay, from the rope breaking, fell backwards off the cart upon the ground, which was hard from a severe frost of several days continuance, and without any covering of snow. On his fellow-labourers hastening to him, he spoke once, and desired them to let him alone. A state of insensibility, with frequent convulsions, immediately succeeded. In this state, Mr Lightfoot, surgeon, was sent for, who had him conveyed about a quarter of a mile to his master's house, and opened a vein. I saw him about an hour and a half after the accident. About 12 ounces of blood had been taken from the vein, which was still open, but the blood flowed very slowly. I had his head shaved, but there was not the slightest apparent injury of the cranium, nor even a bruise upon the integuments. The spine had not suffered any injury. The insensibility and convulsions increased. He moved his lips occasionally, but every tea-spoonful of water, on being put into his mouth, produced general and alarming convulsions, and violent spasms of the muscles of deglutition. His eyelids were closed. When I first examined the eyes, the pupils were dilated, and nearly if not altogether insensible to the stimulus of a lighted candle. On examining them some time afterwards, the pupil was constricted, and not much larger than a pin's head. The latter appearance of the iris continued. I examined the eyes frequently during about an hour and a half. There was not any particular determination of blood to the head. The pulse was 70, full and labouring.

I opened the right temporal artery, and not getting much blood from it, I opened the left, and took about 10 ounces of blood

blood. The vein continued open. In all about two pounds of blood were taken, when the vessels were secured. The bad symptoms continued and increased. The breathing, which throughout had been laborious, became still more so; very irregular, and was performed chiefly by the diaphragm. His pulse was now irregular, but not intermitting; occasionally it was not more than 60, and tolerably firm, occasionally nearly 100, and fluttering. The iris remained as described, and totally insensible to light. He had not any power over the muscles of the neck. His head rolled off as the pillow was moved, and remained in the position into which it fell. His extremities became exceedingly cold, in spite of the application of a large bottle of boiling water to his feet, and though friction with hot flannels was vigorously continued more than an hour upon his limbs. The breathing became more and more slow, and most difficult, with catchings and general convulsions. He appeared sinking very fast, and as if every convulsion might be the last. His joints were stiffening, not with convulsions, but with cold, and as it seemed with death. His nearest relations left his bed-side, to avoid seeing him die.

In these circumstances, I proposed to the surgeon to give him an enema composed of two drachms of assafœtida, dissolved in half a pint of warm water, and whilst he ran to prepare it, I procured a pewter pint syringe. To this clyster we added, in the room, about four ounces of good vinegar. On proceeding to inject it, his limbs were found stiff like a corpse, the hip-joints and knee-joints nearly immoveable. I was struck with the resemblance to a subject upon a dissecting-table. The thighs were gradually separated by force, and the enema given. In a few minutes he began to twinkle his eyelids, and move his lips; he drew a very deep inspiration, and became strongly convulsed. When this subsided, he swallowed with little difficulty a table-spoonful of cold water. He opened and closed his eyelids. On touching the eyelashes, there was evident sensibility. On opening the lids, the iris contracted to the candle light. In the course of ten minutes he spoke, and evidently knew some of the persons round the bed. In answer to my questions, he complained of his head and neck, but particularly of his head. I took the dressings from the left temporal artery, and let it bleed till he expressed himself free from pain in his head, and complained only of his neck. Nothing was observable about the neck. About 10 ounces of blood were thus taken from the artery. In the course of 20 minutes, he returned the clyster with a very copious stool. Whilst borne to the night-chair, he observed the sprinkling of blood upon his shirt, and asked if he had

had had leeches. On being placed in bed again, he evidently knew me, and some little time after called me by my name.

He was ordered to take immediately a bolus with 5 grains of calomel, and 15 of jalap, and an ounce and a half of infusion of senna with sulphat of magnesia, every two hours till it operated. These occurrences occupied from half-past 8 till 10 o'clock.

We visited him again at 12. He had had some slight convulsion, but not much; some sleep; no stool. Continue the infusion.

Thursday 9th, A. M.—A comfortable night, with some sleep. Pulse 70, soft and regular; no pain in the head, some in the neck; tongue clean; no stool. Repetantur bolus et infusum, c. sulphate magnesiæ. Water-gruel *ad libitum*.

Seven P. M.—One scanty stool. Pulse 90; face flushed; considerable pain in the head and neck; eyes impatient of light; skin hot and dry; much restlessness. Says he "is very bad, and has a deal of pain in his head." Tongue dry and rather furred.

Eighteen ounces of blood taken from the arm. Head free from pain after the bleeding; some tendency to syncope. Repeat the infusion, and let him take 12 grains of powder of jalap with each dose, every two hours.

Friday morning, 9 o'clock.—No stool; head free from pain; tongue furred, but moist; some sickness, probably from his medicines, which he has taken regularly every two hours. Statim injiciatur, formâ enematis, solutio sulphat. magnesiæ. Repetantur pulveres et infus. secundâ quâque horâ.

Seven P. M.—Clyster operated well; several dark-coloured stools during the day; evidently convalescent.

Saturday, Sunday, Monday; convalescent.

I saw him repeatedly during the month of March, pursuing his work in perfect health. It may be well to add, that he is a strong plethoric man, accustomed to drink freely of malt liquors, and particularly of porter. He has been successively servant to two dealers in porter. About six months before this accident, he had a severe complaint in his head, and something of a fit, from habitual excess in drinking porter.

In copying this case (from my notes taken at the time,) for insertion in the Edinburgh Journal, it may be well to add, that Firth has continued perfectly well, without the least inconvenience remaining from his accident.

Halifax, September 20, 1813.

III.

On the Use of Charcoal-Powder, as a substitute for Cinchona. By R. Calvert, Physician to the Forces, Palermo.

A Physician to the Sicilian Hospital at Palermo, having hinted to me, that he had prescribed the powder of charcoal in a case of intermittent fever, with success, and recollecting myself that burnt rags, soot, and similar substances, have sometimes succeeded in curing agues in England, I determined to try the effects of charcoal in an obstinate case of ague in my hospital. Finding it succeed in that instance, I have continued to employ it in a variety of other cases. And, was I permitted to form a judgment of its virtues after the short but active period in which I have used it, I should declare it equal, if not superior to Peruvian bark. I subjoin the original case of Dr Calcagno, the Sicilian physician, wherein sulphuric acid was given, with an idea of liberating carbonic acid, or some antiseptic principle which he thought the charcoal contained. As I was not aware of any such principle to be liberated by the combination, I omitted the acid entirely, and without disadvantage, as will be seen in the second case, which was also the next in order of the cases in which charcoal was employed at this place.

The general effects of this substance seem to be, to take away bitter and disagreeable tastes in the mouth; to allay sickness, wherever there is a tendency to vomit, and sometimes to stop the vomiting, when it has occurred; to promote appetite, and assist digestion. It has some tendency, however, to constipate the bowels; but it neither produces griping nor any other unpleasant symptom. In one instance, indeed, where I prescribed it, immediately after the bloody evacuations had ceased in a dysentery of a week's standing, it appeared to prevent the recurrence of that disorder. In any species of fever, after the natural evacuations by the skin and kidneys have been restored, it appears to preserve them in that state, and prevent the fresh accession of fever *. Whether or not the carbon finds its way into the mass of blood, I will not attempt to decide, but certain it is, that the lips soon lose that milky opake appearance, so common in intermittents, and gain a deep redness, while the skin is very soon tinged with the colouring matter, if it may be so called, of the carbon.

* Might it not have a good effect, during the remissions in the plague?

carbon. These two last effects, however, soon go off when the remedy is discontinued.

Should further experience confirm the efficacy of this plentiful, cheap, and well known substance, so as to render it a common substitute for Peruvian bark, it will cause a considerable saving to government. But the greatest advantage will be felt amongst the poor.

I have paid no attention to the particular wood from which the charcoal is obtained; the only selection I have made being of the best charred pieces.

The most convenient mode of administering it is in the form of bolus, folded in a wafer made of flour, after dipping it in water, as is commonly practised in Roman Catholic countries.

I am Sir, your most obedient servant,

R. CALVERT, Physician to the Forces.

CASE I. *(Translation from the original Italian.)*

On the 5th July, a young man named GIACHINO COLOMBA, from Cessalu, aged 20, was admitted into this great hospital; by profession a seaman, of a sanguineous temperament, and tolerably well fed. He had been affected three days with a fever, that assailed him every day towards two o'clock P. M. with a sensation of cold, and that afterwards terminated towards morning with a little perspiration, leaving him free from all those troublesome symptoms, such as thirst, headache, and excessive heat, which tormented him during the course of the paroxysm. He assigned no other cause for his complaint than his employment; but said, that, a few days before, he lived on the shore of Caronia, near a rivulet *. A vomit was immediately prepared of two grains of tartar-emetic, with a drachm of *acetito-ammoniacale*, (acetite of ammonia.) He vomited a quantity of mucous yellow matter. When visited after dinner, the fever was upon him, the cold stage of which, he said, came on at two o'clock, as usual. Decoction of elder flowers; broth, but no food.

6th, In the morning.—Apyrexia. Decoction of camomile; half diet. Fever at 12 o'clock; after dinner the fever continued. Decoction of elder flowers; broth, without food.

He was ordered the powder of charcoal in the dose of a scruple every two hours. To drink after it four ounces of water, acidulated with sulphuric acid; to begin at six o'clock in the morning.

7th, In the morning.—Apyrexia; at 12 o'clock fever. The charcoal

* A situation in Sicily, peculiarly subject to intermittent fevers.

charcoal was suspended; broth, without solid food. After dinner the fever continued; decoction of elder flowers; broth. At 10 o'clock P. M., powder of charcoal and water acidulated, every two hours, as above.

8th, In the morning.—Apyrexia. Continue the charcoal; half diet, with wine. At 2 o'clock P. M. fever; water, broth, decoction of elder flowers. At 10 o'clock the usual powder, in the dose and manner as above.

9th, In the morning.—Apyrexia; half diet, with wine; after dinner, apyrexia; two doses of the powder, as above; diet with wine and an egg.

10th, In the morning.—Apyrexia; meat and full diet; two doses of charcoal, as above. After dinner, apyrexia; two doses of charcoal as above; diet, an egg, wine.

11th, In the morning.—Apyrexia; two doses of charcoal; meat. After dinner discharged from the hospital perfectly well.

(Signed) FRANCISCO CALCAGNO.

CASE II.

JOHN GRAHAM, private 20th Light Dragoons, æt. 45, melancholic temperament, was first attacked with a tertian fever about twelve months ago, when quartered near the *Mole* at Palermo, but not a great while after leaving the *Stradone*, a place notorious for producing remittent and intermittent fevers. He states, that he remained about three months in the hospital of the 3d Battalion King's German Legion, during which time he took a considerable quantity of bark; that in a few days after his discharge, the fever returned, though in a slighter degree, and has hung about him ever since; that he was admitted into the detachment hospital about three months ago, labouring under an inflammation of the intestines, which caused him to lose a considerable quantity of blood (about 90 ounces); that he recovered and was discharged.

On the 14th June last he was readmitted with a gonorrhœa and swelled testicle. These complaints had nearly disappeared, when, on the 16th July, during the morning visit, he was seized with a very strong paroxysm of the ague. Its violence was somewhat interrupted by a dose of opium and the pediluvium. The paroxysm being over, he began to take a drachm of the powder of bark every three hours; low diet, four ounces maccaroni.

17th.—No paroxysm. Continuetur cinchona lancifolia.

18th.—Had a return of the paroxysm at 11 o'clock A. M. with its usual violence. Absente febre, repetatur cinchona lancifolia, ut antea.

19th.—No paroxysm. Continuetur cinchona.

20th.—He took three drachms of bark yesterday, but there being no more in the hospital, he did not get any this morning. At 11 o'clock, the paroxysm returned as before. Sumat pulveris carbonis ℈j. secunda quaque hora.

21st.—No paroxysm. Continuetur pulvis carbonis.

22d.—No paroxysm. Continuetur pulvis carbonis.

23d.—As yesterday. Continuetur pulvis carbonis.

24th.—Continues well. Omittatur pulvis carbonis. Half diet.

27th.—While talking this morning respecting his case, at 11 *o'clock precisely*, the colour was observed suddenly to leave his face, and, when asked what was the matter with him, he said he felt a chilliness down his back, as if the fever was about to attack him. He instantly took a scruple of the charcoal powder with a grain of opium, and the sensation went away. Sumat pulveris carbonis ℈j. ter in die.

August 1st.—Free from complaint. Omitt. pulvis.

Palermo, 1st August 1813.

IV.

Case of Rheumatism of the Heart successfully treated. By James Russell, Surgeon, Birmingham.

The attention of the profession has of late been directed to the tendency to inflammation of the heart which exists in patients who are the subjects of acute rheumatism. We are indebted to Mr Dundas for a paper on this subject, in the first volume of the Medico-Chirurgical Transactions. Dr Baillie also remarks, in his work on Morbid Anatomy, that rheumatism of the heart is a frequent cause of its enlargement; and a very valuable collection of cases of this disease was lately published by Dr Wells, in the third volume of the Transactions of a Society for the improvement of Medical and Chirurgical knowledge. Notwithstanding the number of cases of this affection already before the public, I shall venture to relate the following, as it appears particularly interesting, from illustrating the symptoms of the disease, and from its successful termination.

Seth Bassett, a waggoner, 22 years of age, was attacked with acute rheumatism, about the latter end of May 1810. The pain at

at first seized him in his legs and ankles, but afterwards removed to his shoulders and arms. The attack had been so severe, that the medical gentlemen, under whose care he was, had bled him six times. He was relieved by the bleedings, and considered himself so much recovered, that he was permitted to return home, a distance of ten miles. On the 27th of June 1810, he became my patient in the Dispensary. He complained of pain in his ankle, which was much swollen and inflamed; he had also pain in the left side of the chest; his pulse was full; tongue covered with a white fur; and his skin moist. Twelve leeches were directed to be applied to his side, and his feet and ankles to be fomented with flannels wrung out of hot water; his bowels being costive, he took four grains of calomel, and a solution of neutral salts. On the 28th, the pain in his side was easier, his legs and ankles were more swollen, and exceedingly painful. On the 29th, the pain and inflammation had suddenly left his extremities, which yet remained swollen; but he complained of great distress in his chest; his breathing was short and difficult, but not painful; the heart and large arteries throbbed most violently, and pressure made upon the left side of the chest gave him great pain. On the same side, between the sixth and seventh ribs, there was an inflamed spot on the skin about the size of half-a-crown. His pulse was 62, large and full, and communicated a jarring sensation to the finger. He lay in an horizontal position, and could not bear to be stirred, but was relieved by his head being a little raised. At this time, Dr Male saw the patient with me. Eighteen ounces of blood were taken from him, which gave him great relief; the pulse rose immediately to 70; antimonials and digitalis were prescribed, and the fomentations were ordered to be continued to his extremities. The symptoms were much relieved the two following days, but they returned on the night of the 2d of July. He had pain in his chest; his breathing was short and quick; pulse same as before; his legs were somewhat œdematous. He was again bled on the following morning, and the dose of digitalis increased to twenty five drops of the tincture every four hours. On the 4th of July, his pulse at 88, and full; the symptoms were much less violent. The pain in his side had not returned on the 5th; he breathed more freely, but the heart and arteries still continued to beat very violently; his pulse 92, and full. Twenty ounces of blood were taken from him, and the dose of digitalis increased to thirty drops every four hours. On the following day, he expressed himself to have been much relieved by the bleeding, and he continued free from pain, and better till the 18th, when the pain in his chest, together with the violent action of the heart, again returned. He was very much troubled

troubled with a beating noise in his head. He was now taking forty drops of digitalis three times in the day, without its producing any perceptible effect upon the pulse. He was extremely low, and, for the first time, expressed great anxiety at being bled. Eight ounces only were taken from him, which caused him to faint. The following day, the symptoms were so violent, that it was necessary to repeat the bleeding, and twelve ounces of blood were taken, which produced a considerable abatement in the violence of the symptoms. He appeared to be much less affected by the loss of blood than on the preceding day. He continued to take the digitalis. On the 20th, he was exceedingly low; his pulse now became irregular and intermitting; he was in no pain, but had a sense of giddiness in his head. The digitalis was discontinued, and light cordials were prescribed. The 21st and 22d, the lowness still continued; he lay in an horizontal posture, and was scarcely able to speak; his pulse which was very feeble, now intermitted every third beat; and he appeared to be sinking very fast. He took nothing but a little wine. On the 23d, I found him very much better; he expressed himself quite free from pain; his pulse was become regular, and he breathed freely; his legs were œdematous; he complained only of extreme weakness; he took a little nourishment, which consisted of milk and light jellies; he was able to lie on his side, which eased him very much. A blister was applied to his breast on the 1st of August, on account of slight pain, and was kept discharging with the savine ointment. On the 4th, he was so much better as to be able to sit up; his appetite was daily improving; pulse regular, and he continued to recover. On the 10th, he walked out of doors, and in two months was able to return to his employment.

It is now more than two years since, and he has had no return of his complaints, but he occasionally feels, especially upon any extraordinary exertions, palpitations at his heart.

The symptoms in this young man's case are so striking and so pathognomonic, that there can be no doubt but he laboured under an attack of inflammation of the heart, and that his recovery is to be attributed to the active treatment which was employed. Indeed, when we consider the great importance of the organ that is the seat of the disease, it will be obvious none but the most prompt and rigorous means can be adopted with any prospect of success. That digitalis and the antiphlogistic regimen contributed much to the cure, I have no doubt; at the same time, it will be manifest, on a perusal of the case, that, to the copious and repeated bleedings, the patient owed his safety. Although, in the treatment of cases of this affection, the most rigid lowering regimen is to be insisted upon, blood-letting is the only remedy upon

upon which we can rely with any degree of confidence; and I am convinced from the experience of Bassett's case, and others that have come under my notice within the last four or five years, that great advantage is to be obtained from a bolder use of the lancet than is generally adopted in the treatment of inflammation of vital organs. The case of Bassett also points out to us, that if the affection be recent and violent, we are not to be deterred from the employment of copious bleedings, even though symptoms of debility should appear, and that the abatement of the symptoms ought alone to restrain us in the use of the lancet. If, however, either from previous disease, or from its advanced stage, the debility should be so great as to forbid a free use of the lancet, great relief will be afforded to the patient from small and repeated bleedings, or from the application of leeches and cupping-glasses.

Birmingham, March 1st 1813.

V.

Observations on the use of Blisters in promoting the absorption of Pus in incipient Buboes, and of water in Hydrocele. By P. Johnson, Surgeon, R. N.

Influenced by the favour you have shewn the two or three communications I have hitherto troubled you with. I am disposed to intrude once more, from the supposition that the use of blisters in the two following diseases is not frequent, and that some good might result from their more general employment.

Having long reflected on the length of time, and great quantity of mercury necessary for the cure of buboes, particularly after their suppuration, discharge of their contents, and exposure to the atmosphere, whether opened by art, or from the absorption of the integuments, I have, during the few years I have been surgeon, constantly endeavoured, when unable to resolve these tumors, as soon as pus could be perceived formed, by the use of warm poultices or otherwise, to excite the action of the absorbents in them, and their vicinity, by the frequent application of blisters.

From the several instances I have seen of the good effects of preventing the rupture of the bubo, I will only mention the history of

of one case. A young man, in the middle of January last, complained to me of being affected with severe pain, and extensive inflammation of several glands in the right groin, two chancres on the glans penis, and considerable fever. For five days antiphlogistic remedies were had recourse to; was confined to bed; then pus could be felt on pressure. Mercurial preparations were immediately used; a blister applied to the bubo; dry lint to the ulcers for five or six days. When the swelling began to increase, the mercury was omitted, and warm poultices again applied, which, in a few days, rendered the formation of pus discernible; then the mercury was tried as before; another blister to the bubo; in ten days ptyalism commenced; chancres began to heal, the integuments of the glands were absorbing fast, when a third and fourth blister effected the entire reduction of the bubo. The weather was very cold, and his accommodations not good, therefore the mercury was not pushed to the daily extent it otherwise might; however, under every disadvantage, he was well in six weeks, and doing his duty in the worst weather.

He was blistered only when pus could be perceived. No corrosive applications were tried with the ulcers, from having frequently observed after their use, and for many hours, great irritation of the general system to follow, which lessens the disposition for the absorption of mercury. The ulcers generally heal when the constitution is saturated with that medicine. Had this man's bubo been opened, I am certain he would not be able to do his duty so soon, particularly as I was unable to prevent the formation of matter.

I could bring forward other cases, where the patients were extremely solicitous to have their buboes opened; and, where in the morning, not the least vestige of them could be perceived, where all the pus was absorbed in one night, to our surprise and satisfaction. In these, the constitutions were sufficiently affected with mercury, and their cures were perfect; the distressing and disagreeable discharge of some weeks continuance was prevented; the troublesome ulceration of the groin put a stop to; and a tedious cure, with unsightly scar, particularly in habits predisposed to scrofula, happily removed, with little injury to the general habit.

The second instance I am about to trouble you with, where the use of blisters had a decided advantage, at least in my opinion, was a man about 24 years old, who laboured under hydrocele of the tunica vaginalis testis for two years, which he said had been occasioned by a contusion of the right testicle, in which side the collection of fluid was. In March last he first found himself so far unwell, as to be put on the sick list; the sac then appeared

peared to contain two or three ounces of fluid; he used some cold applications for a few days, with a suspensary bandage, and went to his duty; the pain in his back was considerable, even from the weight of so slight a burden.

On the 2d June, he was again put on the sick report, when the sac appeared to contain four or five ounces of water. I began his cure, by mixing an equal proportion of simple ointment with the blistering plaster, and allowing it to remain on only four or five hours. The irritation was considerable; was put to bed, and had a dose of opium given him. In a few days the contents of the sac were much diminished; a second blister was tried, only left on for three hours; opium given him as before, and in the middle of the month another was applied, which effected his cure, at least for the present. The 23d, he was sent to duty. Cold water was used in these three weeks for five days, which greatly lessened the pain occasioned by the denuded surface.

In the few instances of hydrocele I have seen, little was done or attempted, before the collection of fluid was at least twelve or fourteen ounces; but in this case, where the quantity was small, the method of cure by trocar, seton, or incision, would not be very readily submitted to by the patient, nor perhaps advised by me; therefore, increasing the action of the absorbents appeared the only immediate, and to the man the least apprehensive method of cure.

Spithead, October 24, 1813.

VI.

Case of Amputation. By JOHN RODMAN, M. D. Paisley.

BEING requested to visit Janet Shaw, on the 23d April 1808, in a village eight miles distant, I obtained the following history. It is more than fifteen months since her knee was injured by a sudden fall upon the street, on which the fore part of the joint struck with violence. Swelling, and a feeling of weakness in the part, succeeded the accident, along with pains increased and renewed by walking. Although these effects did not continue regularly to give the same degree of uneasiness, they were seldom wholly absent, and a tumid appearance remained about the joint. At the commencement of this disorder, she was only

19 years old, and, by entering into juvenile amusements, disregarding the consequences of long journies, applying casual remedies, (regarding every morbid encroachment as trivial), the progressive indisposition was greatly neglected.

In this manner, a number of months passed over with the distempered tendency still increasing, till, by leaping from a height, and suffering from a new stroke upon the knee, greater pain, swelling, stiffness, and debility in the joint were produced. It is easy to conceive, from the prevailing character of mankind, that while the diseased disposition was thus provoked, friendly advices, and ignorant assertions, would readily lead her to adopt the use of various means. Many cures were therefore tried without benefit, and when the disease seemed evidently to have become obstinate, she was conveyed to the Royal Infirmary of Glasgow, wherein she was treated for more than two months.

At present it is not quite three weeks since she left the hospital, and the conveyance home distressed her greatly, the distance being sixteen miles. So great is the swelling round the affected knee, that the measurement of its circumference is fully twice the circumference of the other, and this unequal size of the two extends half up the thigh, while all the diseased limb is exceedingly bulky. The pains she feels are excruciating; she has no relish for nourishment; hectic fever is strongly marked; and, not having experienced the enjoyment of sleep for a long time back, her body is extremely emaciated.

It is impossible to give a correct notion of the touch communicated by pressing upon or grasping this swollen thigh. The substance is somewhat firm, yet elastic, yielding a kind of fleshy feeling, that can cause belief of solid or fluid contents; and the glossy skin has no mark of ulceration, except what has been occasioned by seton peas, whose scars were healed long ago. Any attempt to produce flexion of the leg causes such torment that humanity forbids a repetition. My attention was next directed to the precise state of the os femoris, which I had the pleasure to find of its natural size toward the groin. This information, with the general appearances externally, and deficiency of pain at the part, confirmed my opinion that the bone was sound to a short distance below the great trochanter. No alternative seemed now to be left. Agony continued, and death appeared fast approaching; yet still there was a probability that amputation would give a chance for life, and this chance she begged to have.

Early next day, several medical friends accompanied me to the place of her residence, and approved of the proposal to amputate. It was not to be accomplished, however, without encountering troublesome prospects, and the necessity of amputating

ting near the hip-joint was none of the least. Therefore, after applying the tourniquet, it could not be preserved properly in its place; yet compression of the artery against the pelvis being safely intrusted to an expert surgeon, the operation was completed satisfactorily. The short stump appeared uncommonly swollen, the quantity of cellular substance unusually great, and a morbid hue was strongly impressed on all the parts.

Anxious to obtain demonstrative evidence of the nature of the disease, our curiosity was amply gratified by dissecting the limb. A longitudinal incision was made from the knee to the amputated extremity of the thigh, and the soft parts, which presented striking proofs of distempered depredation, were carefully dissected. In this way a large collection of grumous blood was found, surrounding six inches length of the femur, next the patella, and occupying a largely distended space formed by its accumulation. Particles of osceous matter, and a brownish corrupted fluid, were incorporated with this coagulum, and the whole was highly offensive. But the most remarkable circumstance was, that we detected five *ascaris vermicularis* in a sanguinous lump within the knee-joint. Some were shorter than others, yet they were each from three-fourths of an inch to an inch in length, agile and lively, moving about in the dissolved blood with astonishing celerity. The softness of the patella was such as to be easily penetrated with a scalpel; and the carious thigh-bone was broken near to its head, though it appeared healthy for an inch and a half at least, from the place cut through by amputation. The other parts exhibited great derangement of structure.

During the progress of her amendment, adhesion, after the first ten days, went on as usual; swelling of the stump diminished; hectic symptoms subsided: and, as her recovery was completed within five weeks, she was sent, at the end of three months, to learn a sitting employment, with very agreeable proofs of vigorous health.

The deep lodgement of lively worms under a surface through which they could not have been admitted externally, is a strong corroboration, if not a decisive testimony, in vindication of Dr Baillie's opinion, where he states, " that when the whole evidence in support of both hypotheses is compared, the grounds for believing that, in some orders of animals, equivocal generation takes place, appear stronger than those for a contrary opinion." —Morbid Anatomy, Chap. 8.

Paisley, 4th July 1813.

by expiration in a state of foam. Every three or four minutes the viscid phlegm seemed to irritate and throw into convulsions the muscles of the throat, when she exerted her whole force to expel the offending fluid.

The skin was covered with perspiration: it ran over the face in drops. Her pulse was about 100, and sharp. Her respiration was hurried and unequal. The countenance expressed great anxiety; occasionally it indicated extreme agony. She sometimes glanced her eyes through the room: while her countenance expressed a suspicious watchful dread of some impending evil. Except when the irritation and spasms of the muscles of deglutition occurred, the chief uneasiness she complained of was a painful sense of heat in the throat. The giddiness of the head also continued to trouble her. In general she seemed unwilling to speak, and when she attempted it, the tongue faltered, the lips trembled, and her lower jaw did not readily obey the will. Her speech was consequently hurried, and not always very intelligible. She seemed sensible and perfectly conscious of what was passing around her; she expressed an earnest desire that her father and sister should remain constantly by her. At her own request, a little rice was brought, of which she swallowed a small portion, but after an extremely painful exertion of the muscles of the throat. On hearing one of us express a wish to see water brought into her view, she readily complied, and showed great willingness to gratify us, although she was evidently afraid to make the trial. She even put a cup which contained some water to her lips, and attempted to swallow a little, which was instantly rejected with great force by the spasmodic constriction of the muscles of deglutition;—this attempt was also accompanied with general convulsions. When water was brought in her view a short time after, she turned from it, with a countenance expressive of horror.

When the hand was examined, it was found that the wound was perfectly healed, leaving a slight eschar in the skin, but without induration or tumefaction of the parts under it.

She was bled in the right arm by a large orifice, and the blood permitted to flow until faintness was produced; the quantity of blood taken away was about twenty-four ounces. Soon after the vein was opened, she very intelligibly expressed herself generally relieved; particularly of a heaviness and giddiness of the head. The spasms of the muscles of the throat were evidently less violent, and also much less frequent; her countenance became comparatively placid and natural. Before the vein was closed, the pulse was so weak as scarcely to be felt at the wrist. During her

her state of faintness, a vial containing eau du luce was applied to her nose, which instantly produced strong involuntary actions of the muscles of the throat and face.

In about ten or fifteen minutes after the arm was tied up, the morbid symptoms returned, with increased violence; debility supervened rapidly. An enema, with four drachms of the tinct. of opium, was now exhibited. Shortly after, she was suddenly seized with a severe pain and sensation of burning in the regions of the epigastrium and umbilicus, in consequence of which she shrieked out in great agony. She continued to scream for about twenty minutes, and then became suddenly quiet. A warm bath having been previously prepared, she was now put into it. She expressed no horror either at the sight of the water or on immersion. After being removed from the bath, she was put to bed. The system now seemed extremely relaxed and debilitated; the pulse could not be felt at the wrist; the pupils of the eyes were dilated, the countenance was sunk, and the skin covered with a cold perspiration; foam was no longer formed at the mouth,—probably the power of swallowing returned. Her breathing was hurried, and accompanied with a convulsive sob. At a quarter past nine A. M. she expired.

The histories of two cases of hydrophobia have recently appeared in the Madras papers; both terminated favourably.—The chief cause assigned for such unusual success in the treatment of this disease was copious bleeding.

In the above case, venesection would have been adopted, with a much better prospect of success, had Miss D. been largely or perhaps repeatedly bled on the morning of the 15th, when she was first perceived to shiver at the sight of water. The progress of the complaint being so far advanced before venesection was performed, renders it impossible to estimate from this case how far early and repeated bleeding in this disease may be useful.

I am well aware that there is little novel, either in the origin, progress, or termination of this case. To obtain a perfect knowledge of any disease, a regular series of well authenticated facts contributes greatly. The proximate cause of hydrophobia is so obscure, and its symptoms so difficult to subdue, that we cannot be too diligent in collecting plain statements, or histories of cases of it. By carefully comparing one case with another, and, so far as we are able, accurately estimating the efficacy or inefficacy of the treatment, which may have been adopted, we shall learn, even from our want of success, what medicines or plans of treatment ought to be added to the long list of those which are found to be unavailing in this complaint. Future observers will have it

more

more in their power to occupy a new ground, and to attempt other plans of cure.

The history of this case confirms a melancholy fact,—that the bite of a dog apparently healthy, is occasionally followed by hydrophobic symptoms, while the animal who inflicts the wound continues to eat, and particularly to drink, and when its temper is not much altered. The only certain means of preventing this disease, is too generally deemed premature: the friends of the patient are lulled into security, and readily believe, because they anxiously hope, that the dog is not affected with one of the most destructive of all maladies. We may likewise learn, that complete excision of the lacerated parts two days after the infliction of the wounds, is not likely to be generally successful. In this case the wound being situated in a muscular part, an excellent opportunity was afforded of removing the torn integuments and muscles more effectually than generally occurs in wounds of this kind.

Mercury has been recommended both as a preventive and cure of this disease by many respectable names. Although it was here carried to a very considerable extent, and exhibited early, the supervention of the disease was not prevented by it, probably the symptoms were not in the least protracted.

None of the other three individuals that were bitten on the 2d of May have become affected.

Colombo, 27th November 1812.

VIII.

On the Treatment of West India Fevers. By JOHN MACMILLAN, M. D. Member of the Royal College of Physicians, London, and Physician to his Majesty's Forces.

DURING the last three years service in the Windward and Leeward Islands, the interesting situations in which I have been placed afforded abundant opportunities of observing, on a very extensive scale, the comparative merit of the different modes of treatment recommended in the destructive fevers of the West Indies; and having fully experienced the great superiority of the practice of early and copious evacuations, it occurred to me, that it

it might be useful to offer a few cases, illustrating its success, that those who may be entrusted with the serious and responsible charge of sick, in a climate where diseases, from the rapidity of their course, allow but little time for indecision, may in some degree judge of the indispensable necessity of having recourse to more active measures than were formerly employed.

For a long period, under the idea entertained of the contagious typhoid nature of what is termed yellow-fever, wine, bark, opium, æther, capsicum, and other stimuli, have been freely used, and, whenever, from want of success, the contrary practice of depletion had been tried, evacuants were carried to such a timorous extent, through the apprehension of supervening debility, as to be productive of little comparative benefit. A considerable mortality therefore occurred, whichever treatment had been pursued, and the medical officer, from repeated failures, at length became distrustful of the powers of art in checking the ravages of the disease. Numerous dissections, however, of those who died, and appearances carefully noted of late years, more strictly enjoined in military hospitals, have removed the dread of debility, established the inflammatory nature of the fever, and illustrated the necessity of early, and more copious depletion. In most instances, on opening the dead body, marks of inflammation and gangrene were discovered in the coats of the stomach or intestines; inflammation of the brain or its membranes, with fulness and distention of the vessels of the head; or inflammation of the liver, spleen, lungs, or some other viscus. From this tendency of the fever to typhoid congestion and destruction of organs essential to life, large bleedings and brisk purgatives were visibly indicated, and are now very generally employed, in proportion to the violence of symptoms, and with the marked effect of, in most cases, arresting altogether, or converting what otherwise would prove a severe disease, into a mild tractable remittent.

To elucidate this effect of the practice, the following cases are taken, without particular selection, from many others of a similar description; and it may be proper to observe, that they are given as inserted in the Case-Book at the time of admission; that they were treated as ordinary cases coming into the hospital, and without any view to publication. It must be recollected, however, that various modifications of the disease are constantly to be met with, requiring measures more or less active, according to the leading symptoms of each case.

Henry Fisher, 90th regiment, æt. 31, full habit. Admitted August 2d, 1813; was seized with fever last night. Leading symptoms:

symptoms: violent headache and dorsal pains, face flushed, skin hot, much bilious vomiting, pulse full, belly regular.

Detrahatur sanguis ad ℥xxxvi.

Cap. pil. calomel et jalap. No. iv. statim, et post horas tres. mist. purg. ℥ij.

Vespere.—Leading symptoms relieved; bowels freely purged, still complains of headache, but not so severe.

Applic. empl. vesicat. capiti.

Capt. mist. diaph. ℥ij. statim.

3d.—Skin moist, headache relieved, a yellow suffusion over the whole surface of the body, bowels open, considerable remission.

Hab. decoct. cinch. ℥ij. quater indies.

A slight evening exacerbation recurred until the 12th, and on the 17th he was discharged fit for duty.

August 17th 1813.—Benjamin Linica, Royal York Rangers, æt. 30, full habit. Leading symptoms: violent pain in the head, back and limbs, skin hot, pulse full and quick, tongue furred, bowels costive. Was seized with cold shiverings last night, succeeded by these symptoms.

Detrahatur sanguis ad ℥xxxv.

Capt. calomelanos, gr. x. pulv. jalap, ℈i. pro dosi, statim, et post horas tres mist. purg. ℥ij.

Vespere.—Symptoms partially relieved, bowels freely purged, still complains of headache, pulse rather full.

Detrahatur sanguis ad ℥xx. et. applic. emplast. vesicat. capiti.

18th.—Headache gone, symptoms very much relieved, considerable remission.

Bark was given to prevent the recurrence of a slight evening exacerbation until the 21st; and on the 23d he was discharged for duty.

September 12th.—Peter Fellows, seaman of the army ship, Emma, æt. 23, spare habit. Leading symptoms: headache and general pains, face flushed, skin dry and hot, stomach irritable, pulse full, belly costive.

Detrahatur sanguis ad ℥xxxvi.

Capt. pil. calomel. comp. iv. statim, et post horas tres mist. purg. ℥ij.

13th.—Headache relieved, skin soft, copious alvine evacuations.

Hab. mist. diaph. ℥ij. ter indies.

Vespere.—

Vespere.—Considerable exacerbation of fever this evening, skin hot and dry, pulse full, much headache.

Detrahatur sanguis ad ℥xvi.

14th.—Headache gone, skin cool, considerable remission.

Hab. decoct. cinch. ℥ij. quater indies.

An evening exacerbation recurred until the 18th, and on the 24th discharged for duty.

June 16th.—James Smith, 15th regiment, æt. 40, habit rather full, was attacked with fever yesterday evening. Leading symptoms: violent headache, pulse full, face flushed, dorsal pains, bowels costive, tongue foul, heat 95.

Detrah. sanguis ad ℥xxxij.

Capt. mist. purg. ℥ij. 2da q. q. hora, donec alvus rite soluta sit. Ut. baln. tepid.

Vespere.—Some stools from the purgative, symptoms partially relieved, headache still continues, but not so severe; pulse rather full.

Detrah. sanguis ad ℥xij.

Applic. empl. vesicat. capiti, et injic. enema purgans.

17th.—Headache entirely gone, copious alvine evacuations, skin cool, considerable remission.

Hab. mist. cinch. ℥ij. ter indies.

The bark was given for a few days in the remissions, and on the 26th discharged for duty.

April 22d.—William Kite, Royal York Rangers, æt. 35, full habit, sanguine temperament, was attacked last night with the usual symptoms of fever, and admitted into hospital this morning. Leading symptoms: headache violent, eyes red, face flushed, tongue foul, stomach painful on pressure, skin dry, bowels costive, pains of limbs and loins.

Detrah. sanguis ad ℥xl.

℞ Calomel. gr. x.

Pulv. jalap. ℈i. ft. pilulæ quatuor statim sumendæ, et post horas tres cap. mist. purg. ℥ij.

23d.—Frequent stools, pulse 80, heat 95, headache relieved, skin warm, tongue foul, pains moderate.

Capt. solutionis sulph. magnesiæ ℥iss. bis indies.

24th.—Pulse 60, heat 95, headache gone, countenance natural, skin warm and moist, tongue clear at the edges, pains relieved.

℞ Calomel. gr. ij.

Pulv. Jacobi gr. iij. ft. pilula 3tia. q. q. hora sumenda.

25th.—Copious perspiration during the night, leading symptoms relieved, considerable remission of fever.

The bark in the remissions; and on the 8th May discharged for duty.

August 6th.—JOHN SALMOND, 25th Regiment, æt. 21, habit rather full, was attacked with fever last night at 10 o'clock. Leading symptoms: severe headache and dorsal pains, face flushed, eyes inflamed, skin hot and dry; pulse 78, full and strong; cough somewhat troublesome; bowels regular.

Detrah. sanguis ad ℥xl.

Capt calomel gr. x. pulv. jalap. ℈i. pro dosi.

Vespere.—Symptoms much relieved since the bleeding, skin moist, some degree of headache.

Hab. statim mist. purg. ℥ij.

7th.—Much dosing, heaviness of the head, but no severe pain, skin moist, pulse 84, full and soft.

Appl. empl. vesicat. capiti. Hab. pil. calomel, et pulv. Jacobi, ℞tia q. q hora.

8th.—Pulse 76, of moderate strength; bowels freely purged, countenance clear, head relieved.

Hab. decoct. cinch. ℥ij. quater indies.

Bark in the remissions, and on the 16th discharged for duty.

May 28th.—JAMES OGILVIE, 25th Regiment, æt. 24, full habit; time of attack uncertain, having been seized with fever on board ship—used no remedies. Leading symtoms: pulse 96 and weak, skin of natural temperature, tongue slightly furred, thirst considerable, stomach irritable.

Capt. calomel. gr. x. pulv. jalap. ℈i. pro dosi, et post horas tres mixt. purg. ℥ij.

29th.—Some stools from the purgative, pulse 86, soft and weak, tongue moist, skin cool, vomiting frequent.

℞ Mist. camph. ℥vi.
Ætheris nitrosi,
Tinct. opii, ā ā ℥i. M.

Capt. ℥ij. tertia q. q. hora. Three gills of wine.—Frictions of the epigastric region with camphorated spirits.

30th, half past 6 A. M.—Vomiting very frequent during the night, tongue covered with a brown fur, thirst considerable, pulse very weak, extremities cold. Died at 9 o'clock.

Sectio Cadaveris.—Head.—The dura mater inflamed along the course of the longitudinal sinus; considerable effusion and adhesions between the coats, and two or three dark gangrenous spots. Abdominal viscera.—The stomach contained a small quantity of black fluid, and its internal coat highly inflamed, with many

many gangrenous spots. The gall-gladder was full of very black viscid bile.

This man came into hospital too late for the employment of evacuants. The fever was completely formed, and inflammation established. It appeared on dissection in the head and stomach; and the early stage of reaction was past, when active measures might probably have cut the disease short, or rendered it a mild remittent. It had consequently to take its course, and experienced little relief from any remedies.

An immense number of cases of a similar description might be adduced, in illustration of the utility of the practice, from every military hospital in the Windward and Leeward Islands; but I deemed the few foregoing fully sufficient for the purpose of exemplification, and as many as I imagined could conveniently be admitted into a periodical publication.

The following dissections of men who died of the severe form of the fever, or what has been denominated "yellow-fever," I conceive of much importance in elucidating the nature of the disease.

Joseph Lawler, 25th Regiment, æt. 28, full habit. Admitted into hospital 23d April, died 2d May. *Sectio cadaveris.*—The dura mater, along the course of the longitudinal sinus, highly inflamed; the pia mater covered with effused coagulable lymph, the ventricles filled with serum. Abdominal viscera.—The inner coats of the stomach inflamed with small grangrenous spots, and contained a dark-coloured fluid. The gall-bladder distended with black viscid bile.

Samuel James, 15th Regiment, æt. 30, spare habit. Admitted 12th, and died 19th June. *Sectio cadaveris.*—Dura mater inflamed, and adhesions between it and the pia mater; vessels of the latter distended with blood, and an effusion of lymph over the surface of the brain. Abdominal viscera.—Omentum in a state of inflammation; considerable diffused inflammation over the internal surface of the stomach, with dark gangrenous spots.

Corporal Ferguson, 15th Regiment, æt. 20, habit rather full. Admitted 14th, and died 18th June. *Sectio cadaveris.*—Head. Dura mater inflamed, chiefly along the course of the longitudinal sinus; adhesions between the dura and pia mater; the left ventricle full of water. Abd. viscera.—The internal surface of the stomach covered with a dark-coloured matter. Intestines inflamed, and, in some places, approaching to gangrene.

Michael

MICHAEL LARKINS, 25th Regiment, æt. 22, full habit. Admitted 2d, and died 12th May. *Sectio cadaveris.*—Head.—Considerable inflammation of the dura mater, along the course of the longitudinal sinus, but more particularly at its posterior part. The vessels of the pia mater turgid with blood, and several adhesions between the coats. Abd. viscera.—The stomach contained a quantity of the usual black matter; its internal coat very much inflamed, extending over the whole of its surface; the gall-bladder uncommonly large, and filled with black bile.

GEORGE ROSS, 25th Regiment, æt. 32, full habit. Admitted 23d, died 28th May. *Sectio cadaveris.*—Head.—Slight inflammation of the dura mater along the course of the longitudinal sinus, and extensive adhesions between the coats. On separating the coats, two small pieces of bone were discovered about the size of a pin. Between the substance of the brain and pia mater, there was considerable effusion of lymph. Abd. viscera.—The stomach contained a small quantity of black fluid, and considerable marks of inflammation were observed about the pylorus. The intestines were mostly filled with the same black matter as the stomach. The liver had a yellowish unnatural appearance; the gall-bladder unusually small, and containing a small quantity of black viscid bile.

THOMAS HYNES, 3d battalion, 60th Regiment, æt. 23, habit rather full. Admitted 24th, and died 27th June. *Sectio cadaveris.*—Stomach preternaturally red; small gangrenous spots observed on the internal surface of the intestines; liver considerably enlarged.

GEORGE GERCKIE, 3d battalion 60th Regiment, æt. 36, full habit. Admitted 17th, and died 22d August. *Sectio cadaveris.*—An effusion of about six or eight ounces of reddish serum under the dura mater, and in the ventricles. The surface of the brain red and vascular to a very remarkable degree. The lungs of the right side more firm in their substance, and turgid with blood. The liver enlarged, and of a pale diseased appearance. The internal surface of the intestines very much inflamed.

From the preceding cases and dissections, the following facts seem to be established:

1*st*, That whenever copious evacuations are used at the commencement of fever, during the stage of increased arterial action, it is, in most instances, cut short, or rendered mild.

2*dly*, That a greater impression is made on the disease, and more

more benefit derived from one large bleeding, than from repeated smaller bleedings abstracting the same quantity of blood.

3dly, That the debility produced by these large evacuations is not experienced in any degree equal to what might be imagined; on the contrary, more reduction of strength is observed after a few hours violent reaction at an early stage, than from a copious bleeding at this period.

4thly, That when evacuations are neglected at the commencement, they can rarely be used at a late period. The fever must then take its course, and the event has unfortunately been too often fatal with every mode of treatment hitherto pursued.

5thly, That the bark is in no degree useful; on the contrary, does harm, unless the inflammatory tendency be first counteracted by evacuants. This observation applies only to intermittents.

And, *lastly*, That the fever is clearly shewn to be of an inflammatory nature; by the symptoms of strong reaction at the commencement; by its tendency to topical congestion, and destruction of important viscera, as appears by dissection; and by the marked advantage derived from the evacuant plan of treatment.

Whether the yellow-fever, as it is termed, differs in kind or in degree from the common remittent, I do not intend in this place to examine, as I wish to confine myself merely to a short description of the practice found most useful, and to avoid entering upon a subject that has occasioned much controversy, and requiring a length of disquisition totally incompatible with the limits to which a communication of this sort must necessarily be restricted. I may, however, cursorily observe, that I was first disposed to view them as distinct kinds of fever; but more extended experience has inclined me to coincide in the opinion now pretty generally entertained by those engaged in military and civil practice in the West Indies, that they differ only in the degree of severity of symptoms.

This may be judged of more clearly in the wards of a large military hospital, where fever may be viewed in all its degrees at the same time in different subjects, varying from the intermittent to the mild remittent, and, by increasing stages of severity, to what has been denominated yellow-fever. In Europeans newly arrived, of full habit and robust health, the exciting causes will no doubt act with greater force than with those accustomed to their action, and produce the disease in its most aggravated form. The fever is often at first continued; by evacuants breaking the force of reaction, it becomes remittent; and frequently in the same patients, before discharged from hospital, it may be observed to change to an intermittent. From this view of the subject, all West India fevers are supposed to arise from the same

causes, according to the degree of power and intensity of their application, and seasoning of the individual, producing various modifications of the disease; and the idea of contagion is now very seldom entertained.

In all situations where the degree of heat exceeds 80° of Fahrenheit, with marshy or uncultivated lands to be acted upon by this high temperature, similar diseases may be observed. Thus, the troops of the Walcheren expedition, having landed at the hottest time of the year, in a very marshy country, and not being seasoned to the climate, were immediately attacked with the worst symptoms of the endemic fever, whilst the natives remained healthy, and indeed more free from sickness than some years preceding that event.

Having had charge of a great number of the sick of that expedition, I was forcibly struck, on coming out to the West Indies, with the great similarity of the disease; and in my first or second monthly sick Report to the Army Medical Board, after my arrival, I gave it as my opinion that little difference in reality existed *. I was since pleased to learn that the same opinion is entertained by every medical officer who served in the West Indies, and has either seen or obtained a correct description of the Walcheren fever.

Whatever discrepancy of opinion may have existed respecting the nature and causes of West India fevers, it is satisfactory to observe, that the practice which is of chief importance is now pretty generally established. The quantity of blood found necessary to be drawn, may probably shock the prejudice of those who have formed their opinion of blood-letting from the cautious directions given respecting its use in fevers by various writers, and from the sparing employment of this powerful remedy in private practice at home. The most sceptical, however, have only to view the superior success of the practice, or to open a few dead bodies where early evacuations had been neglected, and their scruples must immediately vanish.

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* The Walcheren fever did not appear altogether in that degree of the desease to which the term yellow-fever has been applied, owing probably to the temperature not being so high as to increase the power of the cause to the same degree of intensity with which it acts in the West Indies.

In a paper on the fevers of Sicily, communicated by Mr Bruce, Surgeon to the Forces, in the 30th Number of this Journal, April 1812, a description is given of the fever which prevails in Sicily during the hot season of the year, when the thermometer ranges from 82° to 89°, which corresponds exactly with the yellow-fever of this country in every characteristic symptom of the disease. On dissection the same appearances of inflammation and destruction of organs were observed; but I cannot agree in the opinion that the local inflammation was the origin and cause, rather than the consequence of the fever.

Another improvement, connected with medical treatment, has of late years been effected in the West Indies, by the introduction of a more correct adaptation of diet to the state of disease.

When diet is given to excess, in a deranged state of the digestive organs, it cannot fail to do harm, by aggravating symptoms and counteracting the effects expected from the medical prescription; and its regulation, therefore, is a matter of importance, and deserves much the attention of the medical officer, to whom considerable latitude is given in this respect to exercise his judgment and discretion. An erroneous notion was long entertained, that a full stimulant diet was necessary to support and strengthen the system in what were considered diseases of debility; and under this persuasion, the apparent debility of patients, and the opinion then held of the nature of the disease, large quantities of wine and porter, with animal and vegetable food, were ordered in such profusion, as to incur heavy expense, attended with evident injury to the sick *. During the continuance of this wasteful and hurtful application of diet, Dr Ker was appointed head of the medical department, and by much pains, assiduity, and with some prejudices to overcome, succeeded in establishing more correct views upon the subject.

The utility and importance of the change in treatment, both as to the application of medicine and diet, is now generally acknowledged, and justly appreciated, and the effect of lessening the mortality that has always been remarkable in the West Indies, appears decidedly conspicuous in the sick-returns, the number of deaths bearing but a very small proportion, compared to what formerly occurred when the stimulant practice prevailed.

Barbadoes, October 2, 1813.

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* The quantity of wine now ordered in the General Hospital, Barbadoes, containing about 300 patients, does not exceed on an average one gallon and a half per day, and the success in treatment never was more remarkable. This hospital had always, as well as every other in the West Indies, a great number of chronic cases of visceral obstruction, the sequelæ of remittent and intermittent fevers; but since the practice of depletion at the commencement of fever was more generally adopted, and wine very sparingly given, chronic cases have become comparatively rare.

It is a popular, but erroneous belief, that when a person is weak from whatever cause, an invigorating diet, as it is termed, with a large portion of wine, is required to restore strength. The reasoning is plausible, and it would be found difficult to confute a doctrine apparently so simple, with those not competent to understand the professional principles upon which a reduction of diet in the diseased state of the system is founded.

IX.

Case of Epilepsy, terminating in Death, apparently arising from a Disordered State of the Digestive Organs. By Mr N. H. Clifton, Surgeon, Islington.

A middle-aged man, a carpenter by trade, was, about six months ago, attacked with epileptic fits, which usually occurred once, and sometimes twice in twenty-four hours. Various means were employed, but without apparent advantage; when, about two months after his first attack, they suddenly ceased, and he was enabled to pursue his occupation without any inconvenience, or, to him, perceptible difference, insomuch that he considered himself completely well,—although, upon inquiry, I find that his employer perceived a deterioration of his memory. He continued, however, to consider himself well for four months, when he was suddenly seized with a fit while at work, and fell down. From this time he had a recurrence twice a-day, preceded by a most violent pain from the shoulder to the elbow of each arm, which generally lasted an hour, when the occurrence of the fit procured him complete relief, and left him apparently well; so much so, that the poor man anxiously wished, when the pain came on, for the approach of the fit, that he might obtain ease. He remained in this state for four days, when, during an interval of ease, he fell down in the street, and expired.

On dissection, twenty hours after death, the only circumstances remarkable in the head, were the general plethora of the vessels of the scalp and dura mater; the morbid softness of the medullary substance of the encephalon, together with the paleness of the plexus choroides, and fulness of the lateral ventricles; circumstances which by no means satisfactorily accounted for the cause of death. It perhaps may be right to observe, that the brain was not so much softened as to preclude a correct examination, every part of importance being attended to.

On opening the abdomen, a mass of disease presented itself. The *liver* was considerably enlarged, compact in structure, and livid in appearance; the *pancreas* was likewise enlarged, and very much hardened. The spleen was beset with numerous concretions in the peritoneal investment. The stomach externally was healthy, and free from any appearance of inflammation, while its internal coat was evidently inflamed, and in some parts

parts so much altered in appearance, as to assume almost the character of sphacelus; the mesenteric glands were also slightly enlarged.

The general appearance of the thorax was healthy, the heart being, however, rather larger than usual, and the parietes of the right cavities materially thinner; the left cavities were natural.

It does not appear necessary to add any comment to this statement, except to remark, that, throughout the whole period of this complaint, the patient never complained of pain or inconvenience in any part of the abdomen, even upon pressure; the bowels were regularly relieved; the appetite continued good, and his strength undiminished.

Not having met, either in practice or in books, with a case which establishes the sympathetic action of the organs of digestion, when diseased, upon the brain and nervous system, to such extent as the present, I trust it may not be considered unworthy a place in your Journal.

8, *Lower Terrace, Islington,*
Nov. 16, 1813.

X.

Copy of a Letter from JOSEPH D. A. GILPIN, M. D. Inspector of Military Hospitals at Gibraltar, *to* COLIN CHISHOLM, *M. D. F. R. S. &c. Bristol.*

MY DEAR SIR,—Your letter of April last must have taken a long round before it reached me.

I have lately seen Dr Bancroft's book on Yellow-Fever; and was sorry to observe, that a difference in opinion could be productive of a harshness of language and remark, surely inadmissible, on a topic which merely demands candid investigation, and fair argument.

Of the infected state of the Hankey, I never did, nor ever shall entertain the least doubt; nor do I recollect that any medical man in Grenada held an opposite opinion. During a residence of eighteen years in that island, I had frequent opportunities of seeing the yellow remitting fever; but I have not the least remembrance, that, at any period previous to the arrival of

of the ship in question, a suspicion even was entertained of its being contagious. During a lapse too of so many years, the state of the atmosphere, and the prevalence of marsh miasmata, must have frequently been as they were in 1793, and should consequently (the causes being the same,) have produced similar effects.

Soon after the arrival of the Hankey, a fever appeared, attended by the symptoms so accurately detailed in the 6th chapter of your first volume. Those which always struck me as being peculiar to the disease, were, the appearance of the eye altogether, with the dilated pupil, and exquisite pain felt at the bottom of its orbit. The rapid fatal termination, too, often in 36 hours after its attack, I do not remember to have witnessed in the bilious remittent of tropical climates.

I was not present at the examination of Captain Cox by the Lieutenant-Governor; but I perfectly recollect its result being mentioned to me: namely, that he had kept the clothes and bedding of his passengers.

That those who had visited the Hankey brought the contagion into the town of St George, and that it spread from thence into the country, I have as little doubt as I have of my present existence; and I really believe, that had the most determined Anti-Contagionist been present during that awful period, he would have become a proselyte to the general opinion.

At Martinique, in 1794, we suffered dreadfully from a fever in every respect similar to that which I had seen in Grenada, the preceding year. And in numberless instances, its contagious nature was manifested, by its attacking those who were, *immediately*, about the sick. We had in the General Hospital at St. Pierre's, some hundred cases of it; and it is a melancholy truth, that very few of the attending medical officers survived the pestilent duty in which they were employed.

A most striking instance of contagion, among many others, occurred after the short sickness of the wife of a staff-surgeon. She arrived at Martinique from Barbadoes, and remained on board the vessel a day or two before a lodging could be procured for her. In the mean time she dined with us once at the General Hospital, and returned in the evening to sleep in the ship. The day after, she was seized with the fever in question, and it terminated fatally within 48 hours of its attack. Her immediate attendants were four servants, and I was frequently with her during the day and night. The servants were speedily affected, and died within as short a period as their mistress; and I was the only one who survived of those who were about her person. The captain of the vessel (in which she came) with those

those who assisted in getting her out of the ship, and in conveying her to her lodgings, died a few days after her decease; and I was credibly informed, that, shortly after, the crew died to a man. I lay no stress, however, on this hearsay evidence; what I witnessed myself carried an indelible conviction to my mind of the highly contagious nature of the disease.

With regard to the supposed origin of the fever that raged in this garrison, and which carried off nearly five thousand persons, in the year 1804, I cannot say any thing of my own knowledge, but I have now before me a description of it, drawn up by a very acute and accurate observer, who was on the spot. He was present during the examination of a young man named Santos, who declared as follows: "That he arrived from Cadiz at Gibraltar, on the 25th of August 1804, and that two days afterwards, he was seized with a fever, and that while he was ill, his mother, two of his aunts, a brother, and two sisters, who were in the same house with him, were taken ill also of fever, of which his mother and his aunts died on the 16th of September. He confessed, that, when in Cadiz, he was in a room with a person labouring under a fever." A person of the name of Pratts arrived also in the garrison, who had been ill of fever while living in the same house at Cadiz with Santos. He was the master-cooper of the naval victualling-yard, and of fair character. His examination was taken on oath, and what follows is an extract from it: "That he arrived at Cadiz on the 3d or 4th of August, 1804, and lodged in the tavern Del Sols, where he remained 15 days. He was taken ill of fever, and continued so upwards of a week. That he had black vomiting. On his recovery, his yellow appearance induced a captain of a vessel to refuse him a passage. That the Captain of his own privateer, with himself, lived for many days in the same tavern with a man named Santos; and that he was in the same room with him when he (Pratts) was ill. That while he was in the tavern, several persons were ill: That he was attended after his removal from the tavern by a man to whom he gave some of his clothes; and that shortly after wearing them, he fell ill and died." The examination of Santos, and the above deposition, with the death of those in the house with Santos, convince me that the fever originated in this garrison from him; and from what I have heard since my arrival here, and from what I have seen of bilious remittent fevers, during two summers in the hospitals, I firmly believe, though in opposition to the opinions of very highly respectable medical characters, that the disease in this garrison in 1804 was contagious; and that its

its symptoms were similar to those which attended the fever of 1793, in the island of Grenada.

I know not what might formerly have been the state of the lower lands beneath this rock; but at present, as far as my observation goes, few places are less liable, I should think, to suffer from marsh miasmata, than Gibraltar.

I have thus, my dear Sir, in a few words, given you my ideas on the subject of your letter; and although they may not coincide with the opinions of others, yet, when they are respectively given, candidly and on conviction, they are reciprocally entitled to be *received* with candour.—I am, my dear Sir, yours, &c. Jos. D. A. Gilpin.

Gibraltar, August 12, 1813.

XI.

Observations on the advantages of artificially Scorbuticizing the system in some circumstances. By John Sherwen, M. D.

In the eighth volume of the Edinburgh Medical Journal, there is, at p. 435, a well written case by Dr Jeffreys of Liverpool, in which I feel myself peculiarly interested, and which I think merits the most serious attention of the faculty in general. The circumstances are detailed with so much accuracy and precision, that there can be no difficulty in reasoning upon all the different phenomena which it affords.

Dr Jeffreys has entitled his paper "A Case of Purpura Hæmorrhagica," but, at the conclusion, candidly admits, that it may be considered as a case of scorbutus petechialis; and in this I perfectly agree with him. I think it indeed impossible to account for the following appearances on any other supposition. In the absence of fever, there was an effusion of logwood-coloured spots on the patient's arms, thighs, and body, with similar spots in the inside of his mouth, one of them elevated and spongy, but free from pain; there were profuse and repeated returns of epistaxis; the orifice made in bleeding degenerated into a livid-coloured inflammation; the gums occasionally bled; there were ecchymoses and vibices on the thighs; profuse hæmorrhage from the slight incisions necessary in opening two abscesses. This hæmorrhage was only stopped upon the coming on of syncope; and was afterwards with difficulty suppressed by a refrigerant

refrigerant compress and bandage. In fact, such was the tenuity of the blood and tendency to hæmorrhage, that the slightest puncture with a lancet or needle would probably have been followed by a continued stream of blood. Added to these, the orifice of one of the abscesses was filled with a dark-coloured fungus, discharging a grumous ichor; and again, the next day, on removing the poultice, the discharge had a gangrenous fetor, and the orifice was much distended by a puffy or bladder-like fungus, which, being slightly punctured, discharged a bloody ichor during several hours. Finally, the patient continued to recover, by taking vegetables, tonics, &c.

Such is the detail of circumstances which appear to me to stamp this as a case of true and genuine scorbutus petechialis; and as such, it becomes truly remarkable, not only because it safely demanded, and safely bore venesection to a surprising amount, viz. 86 ounces of blood, independent of profuse hæmorrhage from the nose, &c. having been evacuated in the space of 18 days; but it is also remarkable from the blood abstracted by the lancet having the inflammatory crust or buff on its surface, at least an inch and half in thickness, firm and yellow, far exceeding any thing that the writer had ever seen in the most acute case of pneumonia or rheumatism.

We have here a texture and appearance of blood, which, if the fact had not been here related on such respectable and unquestionable authority, might with difficulty have obtained credit: but it is corroborative of similar appearances, which have long since been noticed, although in a much less degree, by some of the correspondents of the late Dr Lind; and, in the fifth volume of your Journal, by that eminent physician and accurate pathologist, Dr Parry; in two of whose patients he also had observed the crust of coagulated lymph as thick and tenacious as he had ever witnessed in the most acute cases of rheumatism, pleurisy, or hepatitis. This appearance may perhaps be thus easily explained: From the great thickness of the blood, the red globules would subside to the bottom of the crassamentum, leaving the coagulable lymph untinged with red, exactly as the same takes place in all those cases of inflammation where the buff coat appears, and which gave origin to Boerhaave's celebrated theory of Lentor and Viscidity; the theorist not having noticed that what was lentor and viscidity, when cold and congealed in the bason, was the product of celerity and tenuity in the warm circulating vessels.

This copious appearance of coagulable lymph would, at one period of medical opinions, have been thought incompatible with so high a state of scurvy. I confess, that when I wrote my treatise

tise on that disease *, I was decidedly of that opinion; but in subsequent practice I have met with several cases of true scorbutus petechialis, in which the usual degree of debility was wanting, but the tenuity of the blood and disposition to hæmorrhage and fetid ulceration, eminently conspicuous. Some of those cases are given in detail in the Gentleman's Magazine for 1798, pages 105 and 192. Since that time, I have again had opportunities of noticing the same disease in the persons of two gentlemen, who were in the habit of enjoying all the comforts and consequences of affluence and luxury, and unexposed to any of those causes which are too generally considered as alone productive of this malady. But, in every one of these cases, I discovered that the patients had indulged an inordinate inclination for common culinary salt; and there is one expression in the case so well related by Dr Jeffreys, which inclines me to believe, that the disease in his patient had a similar origin. "*He craved much for his usual diet of salt beef.*" Perhaps the same thing may have taken place, though unnoticed, in the few similar cases on record. I suspect this, because, in six cases which have fallen under my observation on shore, the true cause, viz. a superabundance of salt, would have eluded a common inquiry; for, in all the cases, I found a considerable unwillingness to admit the fact; and in one, (an obstinate old medical practitioner), as great an unwillingness to refrain from what he had been in the habit of considering as an indulgence.

One reason for occupying your attention at present is, to request, if it be not inconsistent with the plan of your publication, that the cases of true scorbutus petechialis, which I have mentioned

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* "Cursory remarks on the nature and cause of the marine scurvy, showing that that fatal disease may not only be prevented, but easily cured on board ships at sea."

In an appendix to that essay, I have proposed the use of the broad garden beans, brought into a state of vegetation by maceration in water, to be eaten raw, either by themselves, or cut up and mixed with oil, mustard, and vinegar, as a sallad. By this easy process, the cure may be effected at any period of the longest voyage, and in any climate: but since the discovery of the antiscorbutic effects of that incomparable esculent vegetable called Nopal, in the East Indies, by Dr Anderson, of which there is a concise account in your Medical Journal, Vol. V. p. 481, there can be no occasion, at least not in the Indian seas, for the vegetating legumina; but it ought to be noticed, that these, in their dry state, may be preserved during the longest voyages to any part of the world; and, in the coldest climate may, by simple maceration in fresh water, be speedily brought into that vegetating state in which the germ begins to appear; when they become, to all intents and purposes, recent, wholesome esculent vegetable food; all that is wanted to arrest the progress, and effect the cure of the scorbutus petechialis at sea.

tioned above, may be transferred from the Gentleman's Magazine to your pages *, where they may be likely to fall more immediately under the eye of your numerous professional readers, to whose candid and critical animadversions I wish to see them submitted; as I have there ventured to suggest the probability, in certain cases of *inorganized* polypous concretions of coagulable lymph in the heart or bronchial vessels, of rendering the circulating blood a menstruum for their solution; not doubting but that a free use of common salt during a certain length of time, will be capable of scorbuticizing the system with the same certainty that mercury will produce salivation. Indeed I consider the possibility of scorbuticizing the system by means of common salt, to be by those cases reduced to a certainty, and to be by no means a matter resting on theory or conjecture. How far such a plan may be capable of dissolving a concretion of coagulable lymph in the heart, or bronchial vessels, is certainly at present mere matter of conjecture.

This idea first presented itself in consequence of reflecting on an appearance which I observed in dissecting a favourite dog, which, when seemingly in perfect health, and unagitated by exercise, was suddenly seized with quickened respiration, symptoms of suffocation, and in a few minutes, death. After searching the trachea and bronchial vessels in vain for some foreign substance, I found, on opening the ventricles of the heart, while the dog was yet warm, a plug of *hard* coagulated lymph, about the length and thickness of my little finger, of such a texture as evidently to have been long a resident in that cavity. That such hardened coagula do sometimes form, and long remain, in the human heart, will be evident from the case which I have quoted in the same paper, in the Gentleman's Magazine, from Dr Edward May's very curious letter to Sir Theodore Mayerne, in which he mistook one of these coagula for a serpent; and on which he reasons with wonderful profoundness and ingenuity. A reference to the case of the late Mr Surgeon Holder, admirably drawn up by Dr Cheston in the 6th Volume of the London Medical Journal, p. 225, will show that the symptoms arising from such mechanical obstruction to the free transmission of blood through the heart may, during life, be with some degree of certainty pointed out. Should the obstruction arise from an organized polypous excrescence, little could be expected from the scorbuticizing

* We regret that the great length of the paper alluded to, prevents us from complying with our ingenious correspondent's request, more especially as his ideas are stated with great perspicuity in the present communication.—*Editor.*

scorbuticizing regimen, but far different would be the case on the inorganized coagulum of hardened lymph. We now know that every symptom of scurvy may be very speedily removed by the use of fresh vegetables and ripe acid fruit, so that no permanent injury would be occasioned by the process. We have it, therefore, in our power, by a safe and easy regimen, to throw the human system into that state in which it is well known that the cicatrix of an old wound, and the firmly united callus of a broken bone, may be again dissolved; and if it were possible to ascertain the true pathognomonic symptoms of mechanical obstruction to the circulation from concretions of coagulable lymph in the heart, it will not be an unreasonable expectation that they also may with greater ease undergo a similar but more salutary solution. The existence of the bronchial polypus may generally, perhaps always, be ascertained without difficulty, some portions of them being frequently expectorated by coughing.

The restoration to health from the use of recent vegetables and acid juices of fruit, is, in these cases, so very rapid, that they would undoubtedly in a great measure supersede the necessity of bleeding; but we have now on record the cases by Dr Jeffreys, and Dr Parry, in your volumes, and by Dr Donald Monro, in the Second Volume of the Medical Transactions, p. 328, which demonstrate the safety of bleeding, whenever it may be found necessary under similar appearances: and the following quotation will prove that the same evacuation, and the same antiseptic regimen, may be with confidence prescribed in those cases of purple spots in the small-pox, which, during many years, have been pretty generally found, under a different management, the preludes to a fatal termination. I quote from Holland's observations on the small pox, p. 93. Holland, like Sydenham, is speaking highly in favour of the vitriolic acid in confluent small pox through the whole course of the disease; and he mentions a very extraordinary case, where a large dose was prescribed by Dr Fuller with the most happy event. As the case is short, it merits transcription. "T. T. coachman to Mr Dixon, Recorder of Maidston in Kent, fell ill of a fiery fluxing small-pox. The 10th day after eruption, the fever running high, he fell into a phrensy, and bled at the mouth, nose, eyes and ears. His pox also all about his body bled, and he made bloody urine.

"I chancing to be that day at this town, Mr Finch, his apothecary, consulted me. The case seeming utterly desperate, I desired to be excused; but when he would not be denied, I ordered *plentyful bleeding*, and a powerful styptic and paregoric. These availing nothing at all, we gave forty drops of *oleum vitrioli* in a large vehicle, and half an hour after repeated the same.

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As soon as ever he had taken these, marvellous was the alteration. After he recovered, he remembered he underwent conturbations and struggles unexpressible, and dolours and anguish so intolerable, that, he said, if it were to be done again, he should much rather die quietly, than endure such pangs to save life."

"But immediately all the sluices of the blood were shut up; anon he slept, after awaked perfectly in his senses, without any fever; and from that time gradually amended to a perfect recovery."

It is presumed, that few medical men of the present day would in such a case have ventured to use the lancet: but, when the happy result of this is added to the very important and instructive cases of Doctors Jeffreys, Parry, and Monro; it may not be difficult to believe, that the copious bleeding was by far the most efficient cause of the patient's happy recovery; and perhaps that a free use of the acid of lemons and other ripe fruit, might have answered the same good purpose as the vitriolic acid, without producing the same distressing sensations.

In respect to my proposal for the solution of internal coagulable concretions, by superinducing the scorbutic diathesis, I am aware that it may be objected, in so uncertain a case, as dangerous to expose the patient to so formidable a disease as one by which the callus of a broken bone may be dissolved. In answer to this, I have only to observe, that we every day see patients willingly submit to the loss of limbs, or to the loathsome and painful disease of salivation, for the preservation of life, and restoration to health; and it is well known, that mercury will often effectually cure the disease for which it is administered, when continued a sufficient length of time, without exciting any serious salivary commotion. And such I apprehend would be the effect of the scorbuticizing process. If it should happily succeed, agreeable to my very sanguine expectations, I have no doubt but the salutary solution would take place, long before any serious scorbutic symptoms would arise; symptoms which come on gradually, and which it would always be in our power to regulate and control with far greater ease than we can the effects of mercury. By the intervention of recent vegetables, and ripe acid fruits, or their juices, it will be easy, during any length of time, to keep the patient in a state appproaching to, or slightly scorbutic. And such a regimen may hereafter be found equal to the removal of those indolent glandular tumors, and enlargements of the knee-joint, which are at present among the opprobria of physic and surgery.

There are frequently cases of uterine hæmorrhage, hæmoptysis, and some other anomalous discharges of blood which originate in the same cause as the purpura hæmorrhagica, although unaccompanied by the purple spots; and, in all such cases, the

pleasant antiscorbutic regimen of recent vegetable food, ripe acid fruits, and thin juices, will immediately produce their usual salutary effects, to the exclusion of many other remedies. I have more than once had an opportunity of observing profuse and even dangerous hæmorrhage from the extraction of a loose tooth, to depend upon the same cause.

XII.

Letter inclosing the History and Dissection of a Case, in which a Foreign Body was found within the Heart, to Dr Duncan, Jun. By WILLIAM WOOD, Esq. Fellow of the Royal College of Surgeons, Edinburgh.

DEAR SIR, I inclose an account of the principal symptoms of disease which occurred in a young girl who died, under the care of my father and myself, in consequence of an organic affection of the heart: together with a description of the morbid appearances discovered on dissection.

The symptoms and progress of the disease in this girl, you will perceive, differ little from those generally met with in cases where there has been found, after death, such an alteration in the structure of the heart, or of the large vessels arising from it, as must have disturbed the regularity of the circulation of the blood through that organ; the dissection, however, appears to me to be a singular and interesting one, from its affording an example, by no means of frequent occurrence I believe, of the formation, during life, and existence for probably a great length of time, of loose solid substances, of considerable size, within one of the cavities of the heart.

All the symptoms of the disease, as well as the morbid appearances of the right cavities of the heart, may be, I think, satisfactorily explained on the principle of obstruction to the free passage of the blood through the left side of that organ: and, indeed, the principal difficulty in the case seems to be, in conceiving how the circulation should have been sufficiently carried on to maintain life for any length of time, under such a degree of obstruction as must have been produced in this instance, by the joint effect of the diseased mitral valves, and of the foreign bodies contained in the left auricle, the former materially contracting the opening between the auricle and ventricle, and the latter retarding and diminishing the current of blood towards it; an effect

fect which, from their situation, size, and form, these substances seem to have been well calculated to produce.

With regard to the morbid appearances on the left side of the heart, the most probable supposition appears to me to be, that the disease had originally existed in the mitral valves, producing a diminution in the size of the ventricular opening; that, in consequence of this contraction, accumulation of blood had taken place in the left auricle, and coagula had been there formed, upon which coagulable lymph had been afterwards slowly and gradually deposited; the different substances thus formed assuming different shapes, according to the relative situations in which they had been placed with regard to each other, and to the internal parts of the auricle. But whether this be or be not the true account of the formation of these solid bodies, there can be no doubt, I think, from their laminated structure, that they had existed during life; indeed, upon even the most superficial examination, they are found to be essentially different from those coagula so frequently formed after death in the different cavities of the heart, termed polypi. They are quite different too from those apparently organized solid substances, which have been occasionally met with, adhering firmly to some part of the internal surface of the heart. (Allan Burns on the Diseases of the Heart.)

That cases in which foreign bodies have been formed within the heart during life are of rare occurrence, may be inferred from the circumstance of Dr Baillie not having seen one of the kind. In the fourth edition, however, of his invaluable work on Morbid Anatomy, he refers, in a note, to two cases which had occurred to Mr Brodie; "In some instances," says Dr Baillie, "a coagulum of blood has been found of a laminated texture, in such parts of the heart as are most remote from the direct current of the circulation. This laminated texture shews, that the coagulation had taken place during life, and in a gradual manner. Two cases of this kind have been observed by Mr Brodie, who, although young, is already well known to the public as an excellent anatomist and physiologist. Such cases, however, are to be considered as different from the coagulations which have been generally called polypi, and which fill up entirely one or more of the large cavities of the heart."—(Note to the 4th edition of Baillie's Morbid Anatomy.) From the concise account here given of these substances, they appear to agree with those met with in the present instance, in being of a laminated texture; they differ, however, with regard to situation,—in Mr Brodie's cases having been placed out of the direct current of the circulation, while here they must have been constantly and directly exposed to the stream of blood from the pulmonary veins. Portal makes

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mention of various kinds of substances found within the heart, some of which, he says, are of a laminated texture. He seems to doubt, however, their having been formed during life, although he does not deny the possibility of such a thing happening.

My friends Dr Gordon and Mr Turner inform me, that they recollect seeing, at a dissection in the Royal Infirmary here, five small loose round substances of a rough shaggy appearance taken out of the left auricle, in a case in which there had existed disease of the mitral valves; when opened they were found to be hollow, and to contain a quantity of a whitish fluid.

Unfortunately in this, as in all other cases of organic affections of the heart, neither the history of the disease, nor the examination of the body after death, afford us any practical information whatever; if, however, the case appears to you worthy of being recorded as an object of curiosity, I request that it may be inserted in your Journal.—Yours sincerely, W. Wood.

A. S., a girl of 15 years of age, who had been always of rather a delicate habit, about three years ago, when walking along the street, became suddenly affected with great difficulty of breathing, and general weakness, and was so unwell, that it was with difficulty she could continue her walk home. Next day she was rather better, but ever afterwards remained subject to difficulty of breathing on taking any violent exercise. In addition to this complaint, she, soon after the first attack, became affected with constant strong pulsation in the region of the heart, much increased on walking up stairs or climbing an ascent. It was at this time that I was first called to see her. I found her complaining very much of uneasiness in the chest, from the constant palpitation; she could not bear her clothes at all tight, and she seemed very uncomfortable when the hand was pressed at all firmly on the thorax. The palpitation was so strong, that it could be distinctly seen at a considerable distance. On applying the hand over the region of the heart, strong pulsation was felt; and at a particular spot, about the size of a crown-piece, a peculiar thrilling sensation was communicated to the fingers at each stroke of the heart, not easily to be described, but somewhat resembling the effect produced on the ear by the emission of a short hissing sound at frequent regular intervals. She was thin and pale, but took her food well. The pulse was regular. She could lie down in bed, but her sleep was disturbed by disagreeable dreams. These complaints gradually increased in severity, with the addition of fainting-fits of some duration, occurring generally three or four times in the course of every day. These were sometimes so sudden in their attack, that she dropped down instantly

stantly, deprived of sense and motion; at other times, according to her friends' account, they were preceded by an involuntary contraction of the fingers and toes. These fainting-fits continued for upwards of two months, when they left her, and never again returned. Being removed to the country, I had no opportunity of seeing her for some time, but I was informed, that, while there, having spit up a considerable quantity of a clear watery fluid, after a severe fit of coughing, she had become considerably easier, and could take a little gentle exercise, which she had not been able to do when she left town. On her return, I found her certainly rather easier, but the strong palpitation was still constantly present. This state of comparative comfort, however, was of short duration, for very soon the symptoms became all much aggravated, with the addition of frequent spitting of blood, though in no great quantity. She was now unable to lie down, but was supported in bed by means of pillows, in nearly an upright posture. The pulse was intermittent. She had a very sallow complexion, and was extremely out of spirits. She lingered on, gradually and slowly losing strength and flesh, till the beginning of November, when she died quite exhausted, above three years from the commencement of her complaints. For many weeks previous to her death, dropsical effusion had taken place into the cellular substance of the lower extremities, and by her friends' account, for about fifteen or sixteen days, large purple spots, of the size of half-crown-pieces, had appeared on different parts of her body.

During her illness, digitalis was repeatedly administered, occasionally with the effect of procuring temporary relief; the rest of the treatment consisted in regulating the diet, attending to the state of the bowels, and in occasionally exhibiting cordials.

Dissection.

Within the general cavity of the thorax, there was found a very considerable quantity of bloody serum. The lungs in general were of a healthy appearance, but towards the root of one of the left lobes, there was discovered a considerable mass of a white gritty concretion.

The pericardium, which was somewhat thickened, being laid open, several ounces of clear serum were removed, and the heart brought into view, considerably larger in size than natural, the increase appearing most remarkable on the right side, the cavities of which, particularly the auricle, were much distended with blood. The heart being removed from the body, with the view of having it particularly examined, the right auricle and ventricle were laid open: in these the disease seemed to be entirely confined

ed to enlargement of their cavities. The tricuspid and semilunar valves of the pulmonary artery were apparently in a sound state.

On slitting open the left auricle, which appeared larger than usual, the musculi pectinati were observed to be considerably increased in thickness; and there was found occupying, and nearly entirely filling up the cavity of the left sinus venosus, a round firm body of considerable size; immediately under which, and over the situation of the opening into the ventricle, there was contained another solid substance of an oval form. A third substance, rough and irregular, had also been lodged in the auricle, but its situation could not be precisely ascertained; it lay however towards the ventricular opening. All these being removed, an attempt was made to pass the finger from the auricle into the ventricle, but this was found to be impossible. The left ventricle was therefore fully laid open by an incision, and, on examining the mitral valves, they were found to be much thickened, and quite opake, by their connection and situation permanently contracting the opening from the auricle into a small oval fissure, into which, not even the tip of the little finger could be introduced.

The semilunar valves of the aorta were opake, and much thickened.

The substance, occupying the sinus venosus of the left auricle, when particularly examined, was found to be of a darkish red colour, in form completely spherical, measuring rather more than an inch and a half in diameter. It felt firm, but elastic; the surface was everywhere smooth and polished, but having a singularly clotted appearance. It had no connection with the surrounding parts, rolling loosely in the auricle. When cut open, after having been kept for some days in diluted alcohol, it was found to consist of a sac, one eighth of an inch in thickness, formed of an immense number of firm smooth laminæ, which could be easily separated from each other. Within the cavity formed by this sac, was contained a quantity of coagulated blood. The flat substance which lay between the round body and the opening into the ventricle, was of an oval form, an inch in length, and three-fourths of an inch across at the broadest part. Its upper surface was of the same smooth clotted appearance with that of the former, slightly concave, so as to form a kind of superficial socket for it to roll in; the under surface, which lay over the mitral valves, was rough and irregular, containing something resembling a firm coagulum of blood in a concave depression.

XIII.

XIII.

Pathological and Practical Observations.

(Continued from Vol. IX. p. 448.)

In order to acquire just and accurate conceptions of diseases, and to comprehend fully the nature of those changes which give rise to them, it is essential that we possess an intimate knowledge of the human body in its natural state and healthy condition. This we obtain by the joint aids of anatomy, which demonstrates to us the structure of man's frame; and of physiology, which acquaints us with the living powers it is endued with, and the functions it performs. By the due exercise of these functions life and health are maintained; and, when any material alteration takes place, either in the mode of performing them, or in the effects resulting from them, disease is said to be induced. The investigation of those incipient deviations from a healthy state is the province of pathology, which, if deserving of being considered as a separate branch of medical science, should be deemed a most important one; for, to the point where its duties commence, should all our inquiries respecting the more complicated forms of disease ascend, otherwise our knowledge of their real nature must ever remain vague and imperfect, and our practice for their removal be wavering and uncertain. It is manifest, however, that the changes productive of and constituting disease, can only be judged of by comparison with the pre-existing condition; and, that the processes by which they are effected, can never be explained, or rightly understood, without reference to those healthy actions from which they have deviated. Whence it appears, that pathology cannot properly be deemed a separate branch of medical science; but rather an extension of physiology to the examination of morbid phenomena. Thus too we see, that physiology, taken in its more comprehensive sense, as including necessarily a knowledge of organization, and as essential to the investigation of disease, must constitute the only sure basis of medical practice. Whenever, therefore, speculations are advanced, or opinions brought forward respecting diseases or their remedies, it is our duty to separate carefully the facts from the reasonings, in order to compare the latter, and judge of their consistency with those truths which physiology has established. Now, the purpose of the present communication being to lay before

before the profession some opinions respecting diseases and their treatment, which, I believe, are not generally received, I deemed it adviseable to preface the pathological and practical remarks which I had to offer, by such an outline of the animal economy, as should at once serve to explain and justify the views I had taken.

This I have cursorily sketched in the former part of this essay; and in doing so, I have endeavoured to confine myself to such facts as are universally admitted; to divest the subject of all doubtful or irrelevant speculations; and to represent the series and connection of its several parts in their natural simplicity. I am aware, that I may appear to have left the subject incomplete, from not having assigned some prominent place to the functions of the brain and nervous system. But for this omission and apparent neglect, my reasons I trust will be deemed sufficient. The importance of that energy which is derived from the brain, ganglions, and nerves, though doubtless considerable, can by no means authorize our assigning to them any other place, in the order of animal processes, than that allotted to them by nature. Were our knowledge of their functions as perfect as that which we have arrived at respecting many other organs of the body, we could have no difficulty in ascertaining their several connections, or the place to which they properly belong. But this unhappily is not the case, and a degree of confusion and obscurity has consequently arisen in this branch of physiology, by which the progress of medical science has been much impeded. We know that, on the influence which is derived from the brain and nerves, both sensation and volition depend; but of its intimate nature, or mode of communication, we are very imperfectly informed. The means of forming a natural arrangement of functions have thus been wanting; and a necessity for substituting an artificial one has thereby been induced. That which has so long prevailed, was founded on the basis of relative importance, conformably to which the functions of the brain and nervous system were considered as co-ordinate with those of the heart and arteries. Evidences of their mutual dependence were adduced in proof of the propriety of such distinction; and it was shewn, that, when deprived of nervous energy, the heart soon ceased its pulsations, while a due circulation of blood within the brain was found to be essential to the due performance of its functions. Thus a parity of rank and importance was assigned to each function; and according as the influence of either seemed to prevail in inducing diseases, it was referred to as a primary cause, beyond which it was useless or vain to penetrate.

A bias was thus given to the course of medical reasonings, materially

materially affecting both the theories and practice of the art; for, as diseases could be traced to any obvious or prevailing derangement of either of these functions, distinct and nearly opposite characters were supposed to be impressed on them. So remarkably, indeed, has this been the case, that nervous and inflammatory have, for the most part, been considered as words of opposite import. The treatment of diseases could not but be influenced by such fundamental views; and the effect on that numerous class of diseases generally denominated nervous, has, I fear, not been beneficial. It has not contributed at least to impart steadiness or consistency to the practice in such complaints; for, adherence to system, on the one hand, and the casual lights of experience, on the other, have introduced into the treatment of these, more discordance and incongruity than happily occurs in any other department of medical practice.

When these things are duly reflected on, and, moreover, when it is considered, that the proper object of physiology is not to fabricate a system, however perfect its symmetry, or harmonized its proportions, but to develope the real operations of nature, and to mark the order of proceeding observed in the successive changes constantly going forward within the animal frame, there can be no hesitation in rejecting that division of functions which has so long obtained, as being founded on a distinction utterly valueless, while it diverts attention from the natural order of connective and successive operation, so calculated to afford us clear ideas respecting the origin and progress of diseases. That the obscurity which has hitherto shaded from our view the functions of the nervous system will not always prevail, we have every reason to hope, from the zeal and ability which have of late years been devoted to the investigation. In the mean time, we are bound to abide by what we do really know on this subject, and to avail ourselves of some analogies, which, being derived from incontrovertible principles, cannot well mislead us.

We know that a large proportion of blood, and in its highest state of animalization, is transmitted to the brain; that it is returned therefrom materially changed, and apparently like the venous blood of the rest of the body. The disproportion which the size of the organ bears to the quantity of blood circulating within it, forbids us to believe that the whole of this is destined for its mere nutrition; while analogy, the most direct, leads us to infer that the office of the brain, in the changes which it effects on the blood circulating through it, is precisely similar to that of all glandular bodies; and that the influence therein generated, though so subtile as to elude the examination of our senses, is, nevertheless, strictly of the nature of a glandular secretion.

It

It may be fairly admitted, perhaps, as an accessory argument, that all our reasonings respecting the phenomena of nervous influence, involve its relationship to quantity; and that, when we speak of its accumulation or exhaustion, we virtually acknowledge the existence of a matter generated by the brain and nerves, by whose medium this influence is transmitted. We also, by such language, very clearly admit the necessity for its continual reproduction; and as this can be derived from no other source but the blood, while the brain, ganglions, and nerves, must be the organs effective of such separation; it seems to assimilate the whole process so nearly to that of glandular secretion, as to warrant our completely identifying them. But further light still may be thrown on this subject by some practical illustrations and observations made on disease. For experiments have shewn that diseases manifesting increased nervous energy, have been speedily alleviated by lessening the impetus of the blood transmitted through the carotids, while ordinary experience can sufficiently testify respecting the effects of topical blood-letting in ultimately relieving such. If, then, a due circulation of blood within the brain is essential to the performance of its healthy actions; if an inordinate supply is capable of inducing diseases, marked by an increase of nervous energy; and if, as I believe, can be rendered equally probable, a diminished action of the cerebral arteries, and languid circulation within the brain, produce various nervous affections referable to a state of diminished energy in this organ,—I trust we may deem the analogical proof of its glandular nature as complete, and may conceive ourselves warranted in classing its functions under that order of animal processes, which, in the preceding outline of the animal economy, embraces the aortal circulation. The utility of this altered classification seems to be, that, by restoring this highly important organ to its proper place in the animal system, it brings it back, as it were, within the pale of the constitution, from which its supposed pre-eminence seems, in a great measure, to have excluded it. It leads us, consequently, to refer the diseases of the brain, and of the functions belonging to it, to similar constitutional causes as those which beget disease in other parts; to regard nervous affections less as primary diseases, than as effects secondarily induced, in consequence of disturbed circulation within the head; and to trust more for their removal, to the guidance of general principles, and the employment of means suited to rectifying the errors of the constitution, than to the vague and uncertain operation of supposed specifics.

These few observations I have thought it necessary to make, in order to account for the apparently superfluous procedure of giving

giving the trite detail contained in the former part of this essay; and also to explain the cause of that incompleteness with which it may seem chargeable.

My next purpose is to state some pathological principles, which a contemplation of the animal economy, and an attentive observation of its diseases, have led me to adopt. In bringing them forward, so as to present them distinctly to my readers, and in completing, as far as they extend, the views I have taken, I fear I must render myself liable to the charge of repetition, being necessarily obliged to introduce again to their proper place, observations which have been already advanced in illustration of the natural and healthy actions of the animal frame. In connecting too the healthy and diseased conditions of the body; and in tracing the transition from the former state into the latter, I cannot avoid some recapitulation of the healthy functions also, as a necessary introduction to the consideration of morbid changes; for all which I trust I shall be excused, when it is considered that, in matters of science, brevity is much less an object than clearness of detail, and that, when important objects are concerned, illustrations can hardly be multiplied too abundantly.

In order, however, to attain as much brevity as is consistent with a full exposition of any sentiments, I shall condense what I have to offer on this head, by advancing it in the form of propositions; which I trust will be deemed individually correct, and be found collectively to support those doctrines which it is the object of this paper to inculcate.

The animal frame, while endued with life, is constantly undergoing processes within it, tending directly both to its decay and reproduction, or rather to its waste and repair.

Its continual waste is regularly replaced by depositions from the blood, assimilated to the nature of each structure by the agency of appropriate organs.

As the blood supplies to each part the elements of nutrition, so does it receive back the effete particles of matter into which these become converted, which are returned into it for the purpose of expulsion.

Thus the blood, even in its most healthy state, consists of many heterogeneous components, some of which are nutritive, others excrementitious.

The nutritive particles are supplied to it from the aliment digested in the stomach, and absorbed by the lacteals; the excrementitious are furnished from all the various structures of the body, whence they are taken up, and carried back into the circulation by the lymphatic absorbents.

The

The former are applied to the purposes of the animal economy by means of the nutrient arteries and other secreting organs; the latter are separated and expelled therefrom, according to their respective natures, by the several excretories, namely, the cuticular, renal, hepatic, and intestinal.

From these well established facts it follows, that the proportions of both must be liable to material diversity, according as nutriment is sparingly or excessively supplied, and as excrementitious matter is imperfectly discharged.

The powers of digestion, though limited, are yet capable of supplying a much greater quantity of nutriment than the wants of the system require; their powers may also be so impaired, or their operation otherwise so obstructed, as to leave the necessary nutrition incomplete.

From the view of the animal economy formerly given, it appears, that the occurrence of excessive nutrition is frequent, that of deficient, very rare.

When nutrition is excessive, a state of fulness of blood-vessels is induced, or rather an altered condition of the blood itself; which for a time is relieved by means of the increased energy with which both secretion and excretion are in consequence performed, but which at length gives rise to many diseases.

More especially, however, to diseases characterized in their progress by increased action of the arteries, accompanied by a well known train of attendant symptoms, which, collectively, are designated as a state of fever or inflammation.

It is a well known fact, that the degree of nutriment afforded to the body, depends more on the quality of the aliment subjected to the process of digestion, than to its quantity; and that animal food is more eminently nutritive than vegetable.

It appears also, from much experience, that it is this peculiar diet which more particularly induces a state of plethora in the habit, and which, if ignorantly or wilfully persevered in, aggravates both this and the several febrile and inflammatory complaints which it ultimately gives rise to.

When such state of inflammation arises, some organ is in general more particularly affected; and as this speedily becomes the chief source of distress, it naturally engrosses much of the attention of both patient and practitioner. To its diseased condition the whole train of symptoms is for the most part referred, and from this, though only a single feature of the complaint, and that not of primary existence, a name is affixed, and a character impressed upon the whole assemblage of morbid appearances, which are thenceforward embodied into an individual disease, and assigned an appropriate place in the system of nosology which may chance

chance to be in highest estimation. Both the good and evil resulting from such systems, seem at the present day to be very generally admitted, and clearly understood.

These diseases are for the most part sudden in their attack; that is, the prominent symptom which characterizes and gives name to the combination of morbid actions, is in general more or less suddenly rendered sensible to the patient, who naturally dates his complaints from the period when such symptom is first perceived by him.

It is clear, however, that the moment when sensibility is first excited to the perception of such symptom, cannot be considered as the first of disease. This symptom is oftentimes some acute pain, found on inquiry to arise from a state of inflammation, more or less advanced; which must, therefore, have preexisted, though not attended to until characterized by pain. If attended to at earlier periods, it is highly probable, that the incipient stages of disease would be found distinguished by appropriate symptoms, equally expressive as those which characterize a state of fever or inflammation.

It is also probable, that a correspondent treatment to that which is found most effectual in relieving a state of fever or inflammation, would best alleviate the antecedent stages; and even by correcting the error at its source, avert altogether the consequences that otherwise result from it.

And the doctrine, if true, holds out to us the flattering expectation of being able to strike at the root of a large proportion of human diseases; many of which, and perhaps among the most inveterate, may be distinctly traced to a state of increased arterial action, originally overlooked or neglected; the evidences thereof being subsequently afforded, either by the disordered functions or destruction of parts which the higher degrees of inflammation so eminently occasion, or else by the diseased structure and altered organization to which its less active occurrence so frequently gives rise.

To ascertain the nature of disease, therefore, in its earlier stages, so as to avert, by timely remedies, consequences, which, when they do ensue, are too frequently beyond the reach of art, must be deemed one of the most valuable applications of medical science.

It is manifest, however, that such perfect knowledge of diseases can never be possessed, until pathology shall have succeeded in tracing their progress through every progressive change, from their nascent state to their final termination.

And during this progress we may observe four periods, as more particularly distinguishing the nature of the changes going

forward. To the first belongs the interval between the commencement of a state of plethora, and that of increased action. The second includes that wherein increased action is but irregularly and imperfectly excited; and may be either a simple constitutional disorder, or complicated with incipient disease in some particular organ. The third is marked by actual fever or inflammation, and derives a further character from the nature of the organ to which blood may by such high excitement be more particularly determined. To the fourth may be referred all the consequences which result from such febrile or inflammatory state, when not subdued either by the powers of nature or by medical treatment.

It was in the nature of things, that the two latter stages should have engaged the earliest attention, and have been most successfully investigated by medical inquirers. Nor is it intended, in these remarks, to throw the slightest imputation on our predecessors, to whose labours we are so deeply indebted; and who would have illustrated, with equal ability, the earlier stages of disease, if adequate opportunity for taking cognizance of them had been afforded.

It appears to me, however, that to such early periods of disease, attention may now be most beneficially directed; and that, while we continue to avail ourselves of the accumulated experience of ages in treating the more advanced stages, it is incumbent on us to extend the boundaries of the science we profess, by more completely investigating the earlier approaches; thus to discharge to posterity, some part of that debt which the labours of our forefathers have imposed on us, by illustrating, as far as we are able, those early and important periods of disease, in which medical treatment may be of most avail.

These early periods, as far as they have yet been investigated, are found to be marked by symptoms, indicating an inadequate power of the arterial system; the pulse being, for the most part, low, oppressed, irregular, and intermittent.

A misconception of this state, and misapprehension of its real nature, have given rise to much erroneous practice, and have laid the foundation for many inveterate prejudices. For it was natural, that at first view of such complaints, and when their real nature was misunderstood, cordials and stimulants should be resorted to, as the best curative means for a state presenting such obvious marks of debility.

The symptoms of diminished action in the arteries, and of prostration of strength in other powers, seemed to evince unequivocally a state of weakness, while the effect of cordials and stimulants, in alleviating uneasy sensations, and in giving increased

ed tone and renewed regularity to the circulation, seemed to place the propriety both of the theory and the practice almost beyond a doubt.

And yet there is good reason to believe, that such relief is, for the most part, temporary and fallacious, and that the morbid actions are only altered or repressed, while their cause is still unremoved.

Nor are the phenomena thus produced difficult of explanation; for the effect of stimulants, exhibited under such state of languid circulation, is merely to render the animal powers more equal to the increased labour they are called on to perform, and to re-excite a renewal of those natural efforts, which they had been ineffectually endeavouring to perform.

By which means, however, the symptoms only are alleviated, but the disease not removed.

As originating in a state of plethora, it does not appear how it can be really curable, unless by means of some sort of evacuations.

And it is directly in proof of the doctrine here inculcated, that, in the diseases now referred to, the debility disappears under blood-letting and evacuations, and is even superseded oftentimes by symptoms of increased energy in the arterial system; consequences which, it is apprehended, could not possibly take place if the preceding debility was real.

It is a further evidence, too, that even the most strenuous advocates for such stimulant practice, are obliged to resort to frequent use of purgatives, in order to enable their patients to sustain a continuance of their cordial treatment.

And a confirmation, the most unquestionable, is oftentimes unexpectedly afforded, by the great and immediate relief which spontaneous hæmorrhagy frequently gives to patients who are supposed to labour under diseases of debility, and treated accordingly.

But another source of plethora, or rather of disordered condition of the blood, exists, besides that of inordinate nutrition; for the excrementitious matter returned into the blood, must, if not duly discharged, accumulate therein, and adulterate its nature.

As exercise is the principal stimulant for exciting the various excretions to a due performance of their functions, this source of adulteration may be supposed, more peculiarly, to exist in the sedentary and the indolent.

Both sources of plethora, however, may co-exist in the same body, and be variously combined and modified, according to the degree of each existing; the nature of the constitution they occur in, and the variety of collateral causes by which their natural tendencies may be impeded or accelerated.

Falls

Fully to illustrate the peculiar effects of each species, and to explain all their combinations, must require a long course of observation and experience.

As such, however, cannot be expected to be entered on, unless attention is particularly directed to this mode of investigation, it seems requisite to bring forward such proofs as shall shew, that the views of disease, thus briefly and imperfectly sketched, are at least fundamentally just.

It will go far to establish this, if it shall be made to appear, that inordinate nutrition is a chief source of fever and inflammation; and that the preceding state, though marked by apparent debility, is yet essentially different, and not curable by tonics or stimulants, nor by any means but that of evacuation, under which the debility disappears, though, if real, it should be directly and immediately increased thereby.

And the correctness of such views may, I apprehend, be considered as sufficiently proved, when repeated experience shall have evinced the effects of depletion, not only in removing those symptoms of debility, but even in substituting others unequivocally denoting an opposite state, or one of increased action; and above all, when it shall have traced the transition from this state of apparent debility into one of fever or inflammation, by a progression regular and uniform in its great leading characters, though modified by circumstances peculiar to, or casually affecting each individual.

The term plethora expresses but imperfectly, and not very accurately, the altered condition of the blood referred to in the foregoing pages. Some single term, however, seems desirable; and, until a better shall be devised, this may perhaps be retained without injury. If so, then each species may be distinguished by its appropriate epithet of nutritive or excrementitious.

It is hardly necessary, after what has been said, to remark, that blood-letting is the direct and legitimate remedy for the former; that purging and exercise are most conducive to relieving the latter; and that these several means must occasionally be combined and modified, as either state may most prevail. Hereafter, when this subject shall be more successfully investigated, I should hope, that the symptoms and effects of each state would be so characterized and understood, as to introduce considerable accuracy into the practice required for their removal. At present, it would be premature to enter further into such distinctions, and injurious to the advancement of our knowledge on this important subject, to affect a precision which is not yet attainable.

(To be continued.)

PART

PART II.

CRITICAL ANALYSIS.

I.

Description of the Retreat, an Institution near York, for Insane Persons of the Society of Friends. Containing an account of its Origin and Progress, the modes of Treatment, and a statement of Cases. By SAMUEL TUKE. York, 1813. 8vo. pp. 227.

HOSPITALS for the reception and care of lunatics, deservedly occupy a distinguished place among the charitable institutions of this country, and they have attracted the notice of all foreigners and philanthropists. They confer important benefits on society, by removing many sources of injury and annoyance, which are reciprocally suffered and inflicted by persons whose minds are deranged. They relieve private families from the painful task of attending on a disease, which is rather aggravated than relieved by their anxious desires to render assistance; and they unite in one spot, and under one system of management, all the collective means which have been devised for the relief and cure of this dreadful malady.

By what unknown springs the load which oppresses the mind is shaken off,—by what means the disturbed spirit can be quieted and restored,—is a mystery which cannot be revealed even by those who are accustomed to see such an effect often produced; but the fact is well known, that mental derangement is more speedily and effectually restored in a public hospital than in a private dwelling-house. The disturbance of the intellect is there looked upon as a mere symptom of some bodily disease; it is considered as an effect of some morbid action, like the delirium which occurs in fevers, and thus insanity is deprived of half its horrors. No one shudders at hearing a person talk incoherently whilst he is labouring under the hot fit of a fever, because the mind is regarded as suffering for a season from some disturbance in the organic functions; and it is only by constantly looking upon insanity in this light, that the attendants in a Bethel

spare themselves much uneasiness, and acquire the means of being useful to the sufferer. The horrible character of insanity, and the confused notions and vulgar errors which prevail respecting its nature, seem to arise from its being considered as caused by some secret and mysterious change in the mind, which, by a sort of noble superstition, is exalted above vulgar corporeal organs. It is desirable to teach the imagination habitually to consider the evil as the result of physical causes, and thus prepare the way for the influence of external circumstances, which so often contribute to the alleviation and the cure.

Mr Tuke has given a very interesting description of the effect of local circumstances, and the influence of uniform kindness upon the insane among the society of Friends; and though the method of treatment is conducted upon a scale comparatively small, and confined to one class of patients, yet the practical results are not less valuable. The Retreat at York has now been established seventeen years. It was founded by some enlightened members of the Society of Friends, and by their perseverance and exertions, in the face of difficulties of no ordinary magnitude, the institution has been brought to its present degree of excellence. The origin and progress of the institution is particularly detailed in the three first chapters of this work; an elevation and plans of the building are given, which must be interesting, not only to the subscribers, but also very useful to magistrates and others who may be engaged in erecting similar establishments for criminal and pauper lunatics, as directed by a late act of Parliament. The author, we think, has unfairly charged all public establishments for lunatics, with adopting a harsh and unappropriate system of management, and the expectation he holds out in his preface, of showing the practicability of introducing a more mild system of treatment, is visionary and unfounded. Where has he seen "establishments for the insane poor, whose situation has in general been too pitiable for words to describe?" How did he learn, "that the general treatment of insane persons was too frequently calculated to depress and degrade, rather than to awaken the slumbering reason, or correct its wild hallucinations?" Not surely from the writings of Haslam, Ferriar, Arnold, Cox, or Pinel: nor from a personal examination of the plans pursued at York, Greatford, Liverpool, Manchester, Norwich, London. We are disposed to admit, that the situation of pauper lunatics is very deplorable in workhouses and prisons; but in establishments appropriated for the reception of such patients, we are perfectly sure that blows and stripes have long been discarded, and a system of indulgence and kind-heartedness universally prevails. It is a vulgar error to suppose the turbulence of maniacs is only to

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be checked by severity; the master of every asylum knows, that violent patients are only to be managed by steady and soothing discipline. We have known many persons who have perfectly recovered the use of their intellectual faculties, and returned to their stations in society, deeply impressed with gratitude for the kindness which they received. And what more convincing proof can be given of the kind treatment they have experienced, than this—that many who have been perfectly recovered, have willingly passed the remainder of their days in the asylum, and even asked for such permission as the greatest and most important favour? Whoever has any doubts upon this subject, let him read the account of Dr Willis's establishment, in the *Bibliotheque Britannique*, and of the lunatic asylum at *Closterhayn*, in the 14th volume of the Monthly Magazine.

The chief characteristic of the York Retreat, is the air of neatness and comfort which reigns both without-side and within-side the building.

"There are at present under the care of the institution, 24 male and 98 female patients. The superintendent has endeavoured to furnish a source of amusement to those patients whose walks are necessarily more circumscribed, by supplying each of the courts with a number of animals, such as rabbits, sea-gulls, hawks, and poultry. These creatures are generally very familiar with the patients; and it is believed they are not only the means of innocent pleasure, but that the intercourse with them sometimes tends to awaken the social and benevolent feelings." p. 96.

In the chapters on "*the medical and moral treatment*," the author has recorded facts which are susceptible of some degree of generalization, and on that account they are valuable, inasmuch as they furnish experience for the management of those complicated and fugitive phenomena, which the physician has to observe and to regulate, when the mind and body, taken separately, or in connection with each other, are disordered. Dr Thomas Fowler, the physician first appointed to attend the Retreat, employed various pharmaceutical remedies with sagacious perseverance; but he was satisfied, that the medicinal agents were so imperfectly connected with the progress of recovery, that they were to be considered rather as concomitants than causes.

"There is, however, one remedy which is very frequently employed at the Retreat, and which appears to have been attended with the happiest effects, and that is the *warm bath*. In the first years of the institution, this remedy was not so much employed as it is at present: for it was natural to pay most attention to such means as medical writers, professing experience in the treatment of the maladies of the mind, had most strongly recommended: and it is not a little remarkable, that of the various means proposed for the cure of

bath. This remedy has been for several years, and it still is considered at the Retreat, of greater importance and efficacy, in most cases of melancholia, than all the other medical means which have been employed. Dr Willis appears aware of the importance of warm bathing, in cases of insanity.

"The time of the patient's continuance in the bath, and the temperature at which it is used, are gradually increased; the former from 20 minutes to nearly an hour, and the latter from 85 to 98 degrees. The cold bath has been frequently tried in a variety of cases, both of melancholia and mania; but the result of the experiments is said to be unfavourable to its general use." p. 114.

"Topical bleeding has been found eminently useful, where the approach of a paroxysm was indicated by a determination of blood to the head." p. 117.

"The difficulty of obtaining sleep for maniacal patients, and the unpleasant effects frequently produced by the use of opium, are well known to medical practitioners. It occurred, however, to the sensible mind of the superintendent, that all animals in a natural state repose after a full meal; and, reasoning by analogy, he was led to imagine, that a liberal supper would perhaps prove the best anodyne. He therefore caused a patient, whose violent excitement of mind indisposed him to sleep, to be supplied freely with meat, or cheese and bread, and good porter. The effect answered his expectation: and this mode of obtaining sleep during maniacal paroxysms, has since been very frequently and successfully employed. In cases where the patient is averse to take food, porter alone has been used with evident advantage, always avoiding in all cases any degree of intoxication." p. 118.

Cases of violent mania, we suspect, are not very common at the Retreat. The habit of self-control, which is so assiduously cultivated and acquired by the Society of Friends in early life, probably operates even in insanity, and precludes the patients from ever requiring such a degree of restraint as to prevent the use of considerable exercise, or to render it at all necessary to keep their feet wrapped in flannel. To this circumstance, the author attributes the exemption of the patients in the institution he describes from mortification of the extremities. It is highly to the credit of the superintendents, that no such calamity befalls the patients in the Retreat, and affords a strong proof of the care which is taken of them, and of the excellence of the accommodation. For we attribute many of the accidents which occur in winter, to the bad construction of the cells, and to the total neglect of the proper means for warming them. When hospitals are well built, and warmed by steam, or by heated air, as all such large buildings ought to be, we may hope the sick and bedridden will be spared this additional cause of suffering.

The following is the usual bill of fare for the patients on this charity:

"*Breakfast*—

"*Breakfast*—Milk and bread, or milk porridge.

"*Dinner*—Pudding and animal food five days in the week: fruit pudding, and broth or soup two days. In the afternoon, the men have bread and beer, and the women tea or coffee.

"*Supper*—Generally the same as breakfast, or bread, cheese, and beer.

"The superior patients have no particular diet, but live in all respects as the superintendents. Some of the patients, more especially the melancholics and convalescents, besides their four meals in the day, require the intermediate refreshment of biscuit, with a glass of wine or porter: and attention of this kind is considered almost essential to the recovery of many patients."

Besides a plentiful supply of nourishing food, we heartily concur with the manager of the Retreat, in his estimate of the benefits which air and exercise afford to insane patients. Close confinement, and the want of separation amongst the patients, always appear to us the defects of all large establishments for lunatics, especially in cities and populous towns. We have often observed the happy effect of a long journey on maniacal and melancholic patients, and we believe many persons who are strongly predisposed to attacks of insanity, are enabled to be their own keepers, and escape being shut up in a madhouse, by constantly taking strong exercise, and by keeping themselves in motion, either by walking or riding every day, at all seasons of the year. Horses and carriages are useful appendages to an institution for the recovery of lunatics; and land enough should always be connected with the house, to admit of extensive walks, and to allow some patients to be employed in ploughing, gardening, and in agricultural labours.

The following very curious case, related to Mr Tuke by a medical friend, will serve to show, that, even in idiocy, the mental powers are rather suppressed than destroyed:

"A young woman, who was employed as a domestic servant by the father of the relater, when he was a boy, became insane, and at length sunk into a state of perfect idiocy. In this condition she remained for many years, when she was attacked by a typhus fever; and my friend, having then practised some time, attended her. He was surprised to observe, as the fever advanced, a developement of the mental powers. During that period of the fever, when others were delirious, this patient was entirely rational. She recognized, in the face of her medical attendant, the son of her old master, whom she had known so many years before; and she related many circumstances respecting this family, and others, which had happened to herself in her earlier days. But, alas! it was only the gleam of reason; as the fever abated, clouds again enveloped the mind; she sunk into her former deplorable state, and remained in it until her death, which happened a few years afterwards. I leave to the metaphysi-

From the general tenor of the author's observations, it is evident, that the comfort and happiness of the patients is studied with all the assiduity of kind, yet discriminating attention. How far such indulgencies may be granted with advantage, will be best seen by an examination of the cases, and remarks which the author has given at the conclusion of his book.

"It appears, that from the opening of the institution, in the year 1796, to the end of the year 1811, 149 patients have been admitted. Of this number, only 61 have been recent cases. Thirty-one of these patients have laboured under mania; of whom, two have died, and six remain in the house."—"Of the six cases of dementia, two have died, two have been discharged as not suitable objects, and two remain in the house."

In the first part of this report, there is one material error, probably a typographical one; 21 of 31 maniacal patients are stated to have been so much improved, as not to require further confinement, whereas *two* only can be so enumerated, to make the numbers correspond with the general statement.

The abstract of this statement of 149 patients, will be thus:

Recovered .	56
Improved .	16
Died .	26
Not suitable objects	2
Remain in the house	49
	149

Dr Black, in his arithmetical tables of the cured, incurable, relapses, and deaths, in insanity, which occur in Bethlem, remarks, "*not one half* of the insane are reported as cured; and from these we must also subtract for *relapses*, which, it is probable, would sink the cured radically to 1 of 3." Now, on comparing the results of several public hospitals, we find the proportion of cured to be about that which is stated by Dr Black from Mr Gozna's notes. At Bethlem in London the proportion of patients cured is about 1 in 3, or 2 in 7; at St Luke's hospital, 2 in 5; at the Bicêtre in Paris, 1 in 3; at Berlin, 1 in 2; at the hospital in Cork, 1 in 4; at Norwich, 1 in 3; at the Retreat near York, 3 in 8. Fractional exactness cannot be expected in the documents from which these calculations have been made, but they are sufficiently accurate for general purposes, and the agreement in the results of several large institutions ought perhaps to lead us to adopt the average of the cures here recorded as the general standard. These calculations are sufficient to shew, that there is no marvellous difference in the results of the treatment at different hospitals; and those differences, whatever they may be, are probably

probably owing to some causes which are easily explained, such, for instance, as the admission of recent or old cases; the time of their remaining in the hospital; the rare occurrence of furious mania, &c. We do not see the necessity of attending either to the causes of the disease, or to the meaning of the words *cured* or *recovered*, which Mr Tuke thinks so important in an attempt to draw any inference from a comparison of the number of patients admitted and discharged; because, in the first place, we are wholly ignorant of the causes of insanity; secondly, because little or no difference is perceived in the recovery from the commonly alleged causes; and lastly, because the term *cured* or *recovered* can only be employed when the patient is restored to the state he was in previously to the attack.

After contrasting the preceding statements with the accounts which we have been able to collect from several private asylums, we are disposed to consider the average proportion of the cured to the uncured lunatics, is, as Mr Haslam's observations enable us to infer from the cases in Bethlem, about 1 in 3. The proportion of recoveries at the Retreat is larger than this, but less than at St Luke's and at the Bicêtre, and far below the success which is recorded of the private practice of an individual who is deservedly celebrated for his management of the insane. It was deposed by that reverend and distinguished physician, (the late Dr Willis) that, of patients placed under his care within three months after the attack of the disease, *nine out of ten had recovered*, and also, that the age was of no signification, unless the patient had been afflicted before with the same malady.

From the printed annual reports of Bethlem during thirty years, it appeared, that, out of 6000 lunatics, the deaths amounted to 1200, or about 1 of every 5. The mortality at the Retreat exceeds this; three of the twenty-six deaths recorded were from suicide; but it is very satisfactory to learn, that, in the last eight years, during which the average number of patients has been fifty-six, no circumstance of this kind has occurred.

In an Appendix Mr Tuke has given the testimonies of several respectable individuals who are well qualified to judge of the merits of the establishment, which he so enthusiastically describes. We are ready to allow, that it is one of the best regulated institutions in Europe, but we are persuaded it is not the *only one*; and we sincerely hope that his description will tend to the improvement of them all, and render lunatic hospitals more beneficial to those whose disordered minds can be rectified, and more comfortable to those who cannot again take their stations in the wide world.

II.

A Practical Treatise on the Remittent Fever of Infants, with Remarks on Hydrocephalus Internus or Water in the Brain, and several other Diseases; and Cases and Observations, designed to Illustrate the Influence exerted by a certain disordered state of the Chylopoietic Viscera, upon Local and Constitutional Diseases; and to prove the utility and necessity of Removing it, in order to facilitate and establish their Cure. In Two Parts. By James Millman Coley, Member of the Royal College of Surgeons in London; Member of the Medical and Philosophical Society of St Bartholomew's Hospital, &c. London, 1813. pp. 156.

The disease to which our attention is called by the treatise now before us, is one of the most important of childhood, on account of the frequency of its occurrence, the variety of its symptoms, and the alarming appearances which it sometimes assumes, by simulating the phenomena peculiar to hydrocephalus, a disease in which it does sometimes actually terminate. Yet, alarming as the symptoms of this infantile fever frequently are, it seldom, unless by this conversion, terminates fatally, under proper treatment.

Inappetency, pains in the belly, various dyspeptic symptoms, disordered action of the bowels, picking the nose, grinding the teeth, disturbed sleep, frequent changes of colour, listlesness, and inactivity, with irregular febrile fits, more severe and regular accessions of pyrexia, generally occurring towards evening with distinct remissions; a fever more persistent, with exacerbations more or less marked, accompanied with headache, sighing, moaning, tossing, and restlessness, stupor, &c.;—the sequence of such symptoms, or the concurrence of them, constitutes the most common of all the acute diseases of childhood. Physicians have neither been inattentive to its history, ignorant of its pathology, nor unsuccessful in its treatment. Under the title of worms, worm-fever, the hectic of children, hectic of teething, or marasmus, much useful information may be collected. But, since the time of Dr Butter's publication, physicians have acquiesced in assigning to this disease the name by which it is now again announced to us. Dr Pemberton, Dr Underwood, Dr Hamilton, Mr Burns, and almost every writer on the diseases of children, have added to our knowledge of the remittent fever of infants; so that few

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of the diseases of children are, we believe, better, or more generally understood than this, and, we may add, few in which the practice is more uniform, or more successful. Still our experience of this disease, the result of many years observation, had satisfied us that a rich field was yet open to inquiry, and we consequently rejoiced at the appearance of another monograph on the disease. Our expectations, however, have been somewhat disappointed.

To the history of the infantile remittent given by Dr Butter, little or nothing has been added by Mr Coley. His description is good, for it is transcribed almost verbatim from Dr Butter, without the addition of one new or important observation to its history. He has remarked, indeed, some rare occurrences, such as that of petechiæ in two cases, in the advanced period of the disease, and the discoloration and separation of the epidermis in others. Dr Butter, it is well known, divided this disease into three varieties. The justness of this distinction seems fully acknowledged by our author, though he thinks they may, with more propriety, be called stages;

"because we commonly see them all progressively appearing in the same subject, where the visceral derangement has been extreme, and the illness of long continuance. For instance, while the action of the liver alone is disordered, the state of the patient will correspond with what he calls the 'slow infantile remittent fever;' when the intestines also become diseased, the state will be such as he has described the 'acute infantile remittent fever;' and when the nervous system has become affected by sudden sympathy, or by the debility consequent to long-continued vascular action, the 'low infantile remittent fever,' will be found to prevail, and the disease to have attained its height of violence. In the first instance, the accompanying fever will be found to resemble the *hectic;* in the second it will be of the nature of *synocha;* and in the third a true *synochus*, or what many practitioners might perhaps distinguish by the name of *typhus*. Hence, according to the particular state of the patient, or the opinion of the physician, the disease has been variously denominated: being by one named 'hectica infantilis;' by another 'febris ardens continua infantum;' and by a third, 'febris synochus puerorum.' From the same source it has also derived various other appellations."

But it is in the pathology, and in the treatment of infantile remittent, that we are more particularly called upon to estimate the value of Mr Coley's contributions. It may be well, however, just to state briefly how the doctrine and practice stood previously to the appearance of our author's work. We believe, then, in the first place, that all writers on the infantile remittent, and most practitioners of the present day, have agreed in refer-

ring this fever to irritation originally excited in the *primæ viæ*, to a disordered state of action of the bowels, or chylopoietic viscera. The torpor of the bowels in some cases,—their irregular and inordinate action in others,—the unnatural appearances of the stools, whether spontaneous, or procured by purgatives,—the impeded digestion of all aliment,—the altered secretions,—have all been adduced as proofs, and carefully distinguished:—on these appearances, as indicating the true nature of the disease, the practice, singularly uniform, has been founded. The general treatment consists in the administration of purgatives. Some difference may occur amongst different physicians in the choice of purgatives, and in the extent and manner of exhibiting those remedies. Indeed, it is proper, and often useful, to vary these in different cases, and even in the course of the same case, according to the varying appearances of the disease; rhubarb and magnesia, castor oil, jalap, aloes, the saline purgatives, scammony, gamboge, and calomel, are all in use, and sometimes judiciously, and sometimes very improperly administered. But we are convinced, from all we have observed, that calomel is of all others the most generally, we do not say always the most pertinently, used. The ease with which it is administered to children, a sort of empirical routine, the real merits of the remedy in many cases, the encomiums bestowed on it by Drs Armstrong, Clark, Cheyne, Burns, and other writers on the diseases of children, have all corresponded to introduce it as one of the most general remedies in the treatment of those diseases. We seriously believe that more calomel is consumed by children under nine years of age, than by the whole human race who have passed that period of their existence.

We are very far from thinking that purgatives or calomel are the only remedies required in the treatment of infantile remittent. We know from long and extensive experience, that other remedies may be beneficially called into aid; that antimonials, and fomentations, and frictions, are highly useful; that cordials and tonics are sometimes necessary. We are convinced, too, that the milder purgatives are often to be preferred to the more drastic; and that they are often advantageously combined, or alternated with opiates, contrary to the testimony of our author.

But to return to the pathology of the disease. We have stated the general opinion entertained by physicians. It is, indeed, vague and indefinite, this expression of disordered action of the bowels and chylopoietic viscera; but it serves to regulate the practice, and it connects the series and sequence of the symptoms. Let us see how Mr Coley has improved it. The proximate cause, as he chooses to call it, is said by him to be a torpor, or defective

defective action of some part, or of the whole of the chylopoietic system, and generally of the liver, stomach, and intestines.

" This torpor allows the contents of the two latter to undergo such chemical changes in their nature and composition, as to become the source of extreme irritation to the nervous fibrils that surround them. By the great sympathetic nerve the irritation is communicated to the brain; when febrile action and all the phenomena of nervous and vascular excitement commence. This peculiar susceptibility in the nervous system, or the disposition in the brain and nerves to sympathize with local irritation, which prevails during the state of infancy and childhood, is abundantly evinced by the process of dentition; during which some general disturbance results from the formation and evolution of the primary teeth in most children. Hence appear fever, eruption, coughs, diarrhœa, convulsions, hydrocephalus internus and death. That this susceptibility is comparatively greater during infancy, than at a more mature age, is evident by the absence of every kind of constitutional or general disorder during the formation and appearance of the last molar teeth, which are seen about the period of puberty. At the same time that the general nervous system is thus exempt from disordered sympathies and associations during this process at the latter period; it is surprising that the sensation of the pain, or the local irritation in the gums and alveolar processes, is infinitely greater than during a state of childhood. By means of this nervous susceptibility, I imagine, we may also account for the presence of more general affections of the brain, or of some particular nerves during infancy, from disorder in any part of the digestive organs.

" In the course of my practice I have found affections of the nervous system more frequent and evident, when the liver has been principally disordered, than when the intestines alone have experienced a morbid alteration. This condition of the liver may be ascertained by an examination of the feces, to which recourse should be had in every doubtful or important case. They will be found to be either of a gray or dark clay-colour, or nearly white; from which we may infer that no healthy bile has been mixed with them, or that none has been secreted. This state of the evacuations almost always prevails, where there is a temporary paralysis of the voluntary muscles; as of the organs of speech, those concerned in locomotion, &c. In this situation the patient is sensible, and has only a moderate degree of fever of the hectic nature. This torpid state of the liver does not continue a great length of time without communicating a corresponding alteration to the stomach and bowels, which according to the circumstances is followed by Synocha or Synochus, and an alteration in the evacuations, differing from that which happens when the functions of the liver alone are disturbed. The whole tract of the intestinal canal is diseased; the evacuations from it are black or green; have no resemblance even in consistence to healthy feces; and have not

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undergone that change, which, during their progress through the larger intestines, ought in a state of health always to occur.

The first proposition in this theory, our readers will readily perceive, is merely a change in the usual language; it is the vulgar theory expressed in more fashionable terms. Mr Coley is a zealous disciple of the hepatic school. He has probably been a pupil of Mr Abernethy, but he cannot more admire the talents of that distinguished surgeon, nor more highly appreciate the merits of his researches than we do. When Mr Abernethy, with the hand of a master, traced the connection between local diseases, and constitutional derangement, and shewed how the former might be relieved and cured by improving the general health of the system, when he called the attention of surgeons to the symptoms of disorder of the digestive organs, and demonstrated to them the happy result of a practice directed to the deranged action of the chylopoietic viscera, he enlarged the field of pathological inquiry, and practical knowledge. But it really does not appear to us, that our notions of the pathology of the remittent fever of children is at all extended, or rendered more definite or practical, by this flimsy adaptation of Mr Abernethy's language. Nay, the change of terms is here worse than unproductive. There is, in this disease, we apprehend, something more to be observed than mere torpor, or defective action in the chylopoietic system. There are changed and morbid actions:—nay, there are irritations, and increased actions. If costiveness and inobedience of the bowels to ordinary doses of purgatives be proofs of torpor or defective action, the very frequent dejection of slimy stools are indications of excitement, increased action, and altered secretion. If clay-coloured or whitish stools, displaying an absence or deficiency of bile, be proofs of torpor of the liver,—black, fetid, green, and bilious stools, are surely indications of excitement, and morbid action.

There is something imposing in the simplicity of theory; and, if it be dressed up in a few demi-scientific words, it is a mighty easy matter to give it currency. The annals of medicine are rich in proofs of this easy imposition. The vulgar observe the very unnatural appearance of the stools as well as we do; they tell us how dreadfully the child's bowels are disordered. Physicians say that this proceeds from disordered action of the bowels, and of the chylopoietic viscera; and Mr Coley, with the semblance of more science, that the proximate cause is torpor of *some part* or *of the whole* of the *chylopoietic system*, and *generally* of the *liver*, *stomach*, and *intestines*, which at least absolves the spleen from any share of blame.

Indefinite as all those enunciations of a mere matter of observation

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tion are, we prefer disordered action, to torpor or inactivity of the chylopoietic system, because it has less pretensions, is more comprehensive, and, we think, more just. The actions of the chylopoietic viscera are in this disease changed, disordered, different from what they are in health,—but they are not torpid. We are decidedly of opinion, however, that no enunciation of this kind can advance us beyond the very threshold of pathological inquiry. It is required to know something of those altered actions which do occur. And we sincerely regret, that our author has not attempted to lead us to the true source of discovery, by one single dissection. It is true, that, since he improved the practice, he has not lost a single patient who laboured under this disease; but he confesses, with commendable candour, that before he had changed his mode of treating the disease, he had lost many patients, which, according to our own experience, and the testimony of writers, is not common. It is true, also, that he has entered a sort of caveat against such inquiry; for he has taken care to tell us, that the torpor of the liver implies no organic lesion. We are still persuaded, that dissections would throw light on the pathology of this disease, as perhaps on all others; so at least we may judge from the rich harvest that has been reaped by those who have laboured this field of inquiry.

But, as hints to future inquirers, we may just remark, that we believe that there will be found in the acute forms of the disease, something like inflammation of the mucous membrane of the intestines. The frequent slimy stools, (proofs of increased mucous secretion,) are not unfrequently streaked with blood; and the abdomen feels commonly very hot, and is often distended, tense, and painful. But Mr Coley will see nothing beyond the limits of his own theory. This appearance of blood, like that of petechiæ, he regards merely as a proof of extensive torpor, debility, or relaxation. He is equally disposed to deny the existence of spasm in the intestinal canal. We do not say, with Dr Butter, that this spasm is the proximate cause of the disease; but we are pretty well convinced of its occurrence in many cases of infantile remittent fever. Often, when nothing but scanty, slimy, or green coloured gelatinous stools have passed, notwithstanding the regular exhibition of various and powerful purgatives,—when the abdomen felt tense and swelled from flatulent distention,—we have seen the most decided advantage from warm fomentations applied over the belly; much flatus has escaped *per anum*; the tension of the abdomen has subsided, and a more gentle laxative has occasioned a more free and copious feculent, sometimes scybalous discharge. That the actions of the liver are not those of health, is sufficiently evident from the altered appearance of its secretion.

secretion. We can hardly doubt, that dissection might discover some traces of disease in this organ. We know that, in children who have died of hydrocephalus, a disease in which the stools much resemble those passed in this fever, the liver has borne evident marks of vascular excitement.

The remote causes of infantile remittent fever are probably many. Those which have been most commonly assigned, are teething, worms, improper food, neglected bowels, cold, and moisture, and the sequelæ or dregs, as they have been called, of preceding diseases. We believe that this is as true and full an enumeration as can be given. Some of the remote causes assigned by Mr Coley are far from being so satisfactory. Thus, that hereditary predisposition should be mentioned as a cause of an acute disease so common amongst children, appears to us very surprising. That this fever may occur as a sequela of measles, or of other preceding diseases, we can easily believe. But we have never seen the slightest ground for suspecting that the infantile remittent fever is itself a contagious disease. Mr Coley, however, observes, that the disease has in many instances appeared to have been communicated from one to another, in the same manner as a contagious distemper; and that he has often known parents affected with synochus or typhus, just as their children have been recovering from the worst stage of a remittent fever, which had appeared in them also in the regular succession of contagion. But children are liable to other fevers as well as to this called infantile remittent. They enjoy no immunity from the contagion of typhus, but are often the subjects of that fever. And we are inclined to suspect, that in those cases alluded to by Mr Coley, in which parents and children were successively affected with fever, in the regular succession of contagion, as he calls it, the disease was no other than continued fever, synochus, or typhus, that it was not the fever described in the treatise now before us. Mr Coley's statement and opinion would assert more than he probably intended, and would go to prove, not only that infantile remittent fever may be communicated by contagion, but that the contagion of this fever is identical with that of typhus. We suspect, also, that Mr Coley's attachment to infantile remittent, has, in other instances, led him into similar mistakes. For example, on the 30th May 1811, J. Littleford, four years of age, was taken ill with scarlet-fever; and on the 2d of June, that is on the 3d day of scarlatina, he detects the appearance of remittent fever; and on the next day, or 4th day of scarlet-fever, the symptoms of remittent fever became evident; and this case is given as an example of remittent fever following scarlatina.

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Now, we surely do not hazard much in saying, that this appears quite inconsistent. The scarlet-fever, which began on the 30th May, had not run its course, and attained its crisis on the 2d and 3d of June; on these days, and for five days after, the patient must still have laboured under scarlatina; and, in our opinion, he did continue under the influence of that fever to the 10th of June, when he began to convalesce.

We come now to the treatment of infantile remittent fever, recommended by Mr Coley. We have already stated the plan of cure in general use, viz. the exhibition of purgatives, and particularly of calomel, than which no remedy has been more commonly employed in the treatment of the diseases of children, as a purgative and alterative. In disorders of the bowels, it is prescribed daily by physicians, not so much as an evacuant as an alterant, as a remedy capable of inducing healthy action in the liver and bowels, and of changing the morbid appearance of the stools. "I find nothing so cleansing to the bowels of children as calomel," says Dr Armstrong. It is particularly recommended by Dr Clarke of Dublin, in small alterative doses when children are troubled with green stools and griping; and he observes, that, in the course of a week or two, the stools became natural, although not more than four or five grains of the remedy have been used. In the diarrhœa which takes place after the weaning of children, Dr Cheyne recommends small doses of calomel as the most effectual means of curing the flux. In speaking of the bowel-complaints of infants, Dr Underwood observes, "calomel often proves a sovereign remedy, and may be given in the dose of half a grain or more to infants in the mouth, for two, three, or four successive nights. It is certain," he continues, "that this mineral has a marked operation on the liver, obviating morbid action, and as a stimulant when inert; and is an equally well known deobstruent for all the glandular system, and will be found a very safe and effcacious remedy, &c." We might multiply our quotations to this effect, were it necessary to multiply proofs of what is so well known, the different indications with which calomel is employed in the treatment of the diseases of children, and particularly its daily use with the view of improving the appearance of the stools, and lessening their frequency, by its alterative operation on the liver and chylopoietic system. But, though calomel has been much used also in the treatment of infantile remittent fever, in so far as we know, this has generally been more with a view to its purgative than to its alterative effects. We wish to give to Cæsar the things that are Cæsar's, and we willingly accord to Mr Coley, the merit

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of being the first *author*, who has distinctly pointed out this mode of exhibiting mercurials in the infantile remittent fever.

" The intention with which most practitioners exhibit mercury in this disorder, is that of exciting a strong peristaltic action in the intestines; but it will be found to exercise a still more speedy and beneficial influence, when given with the view of its being absorbed into the system: when it rouses the liver to secrete its bile, the stomach to prepare the gastric juice for the purpose of digesting the food, and the intestines to convert that digested food into chyle, for the nourishment and repair of the enfeebled and emaciated frame. With this design, I have, in general, continued it nearly through the whole of the illness; and have frequently, when it was requisite to produce a purgative operation, given four or five grains of the submuriate every other day, or oftener, for several weeks in succession; not only without any inconvenience, but with the most manifest advantage. Although my practice has been so bold and decisive, I have not met with a single instance of ptyalism nor of mercurial erythema, and the debility consequent on the evacuations has been incomparably less than would have resulted from the natural progress of the disease, or the common mode of treating it. That a return of the healthy action of the digestive organs, a circumstance most desirable in this complaint, may, with justice, be attributed to the agency of this medicine, I think this fact will testify; namely, that in some very obstinate cases, in which, either from defective absorption, or from its passing off too rapidly from the bowels, it has not produced any salutary change after a long period of time; I have been induced to give half a grain, or a grain of the submuriate, or a smaller dose of the oxyde twice daily; which, at the end of three or four days, has generally effected a secretion of bile, removed the unnatural appearance of the feces, and been speedily followed by a re-establishment of health.

" In simple cases of this disease, I know that the employment of purgatives alone will succeed; but, in most of the cases requiring my assistance, there has been such an extreme torpidity of the chylopoietic viscera, as to render the use of mercurials necessary. In these instances, nothing, with which our art can furnish us, could have saved the patients from destruction, had not the most vigorous measures and firm conduct been pursued.

" As soon as I have visited a patient ill with this fever, I direct a dose of Hydrargyri submurias to be given, containing from one to five grains, according to the age and constitution of the patient, the severity of the attack, or the state of the bowels. Two or three hours having elapsed, a draught, composed of sulphate of magnesia, tincture of jalap, and infusion of senna, is administered, and repeated every two hours, until a copious evacuation takes place from the bowels; which I always carefully inspect. After this, the same dose of the submuriate is repeated every second night, and the draught

dranght the following morning, so as to produce several evacuations; until it be ascertained that the digestive organs have regained their natural energy. This effect will sometimes happen in a few days, at others several weeks will elapse before any favourable change will occur. If the constipation be very great, the aperient draught should be repeated every morning, or a dose or two of sulphate of magnesia, or of any other neutral aperient salt; so as to occasion one or two evacuations daily from the bowels. When the feces have become healthy, they are found to be of moderate consistence, having some impressions of the larger intestines upon them; to be of a yellow colour resembling powder of rhubarb; and free from mucus and all other matter that is unnatural to them. As long as they seem to have undergone no change in their passage through the cæcum, colon, and rectum, which is known by their fluidity and heterogeneous consistence, I direct the mercury to be given in a *large* dose, in conjunction with, or followed by a purgative, in the manner above described; but after the bowels have resumed their functions, and the only alteration in their contents is found to consist of a preternatural appearance as to colour, I order the former medicine to be repeated once in twelve hours or oftener, according to the urgency of the case, in small doses; by which means, at the end of a few days, or a week, a secretion of bile takes place, and the disease is entirely removed."

This plan of treatment seems to have been very successful with Mr Coley. He is too judicious a practitioner, however, to trust any case solely and empirically to the alterative powers of calomel. Other purgatives are occasionally prescribed; and he closes his treatise on the infantile remittent, with some very sensible observations on the other remedies.

III.

A Practical Synopsis of Cutaneous Diseases according to the Arrangement of Dr Willan, exhibiting a concise View of the Diagnostic Symptoms and the Method of Treatment. By THOMAS BATEMAN, M. D. F. L. S. Physician to the Public Dispensary, and to the Fever Institution. London, 1813. pp. 342.

UNTIL Dr Willan wrote, there reigned an inconceivable confusion in regard to the diseases of the skin. Little discrimination was employed in distinguishing them, and no atten-

tion paid to precision of nomenclature. Hence, as Dr Bateman, from whose very judicious preface these remarks are borrowed, observes, the words leprosy, scurvy, herpes, scabies, dartres, and some other appellations, became so indefinite, as to be merely synonyms of cutaneous disease. Even the more scientific inquirers did not agree in the meaning of the terms they themselves used, or in the interpretation of those used by the ancients. They not only differed in their acceptation of general terms, such as *pustule*, *phlyctæna*, *exanthema*, *erythema*, *phyma*, *phlyzacium*, &c. but the particular appellations, lichen, psora, herpes, impetigo, porrigo, scabies, and many others, were arbitrarily appropriated to very different genera of disease. The practical evils of such confusion in the use of terms are great and obvious. The experience of our predecessors is either altogether lost to us, or the grossest misapplication of their precepts is the frequent consequence.

"I am aware, indeed, that there are many individuals, professing themselves to be practical men, who affect a contempt for all nosological disquisitions, and deem the discussions relating to nomenclature, in particular, very idle and frivolous, or, at the best, a sort of literary amusement, which is not conducive, in the smallest degree, to the improvement of the medical art. But this I conceive to be a mistaken view of the subject, originating perhaps from indolence, or from a want of habitual precision in the use of language. The inferences of slight and superficial observation may, indeed, be detailed without recourse to a very definite vocabulary; for where little discrimination is exercised, very little nicety can be requisite in regard to the import of the terms in which it is recorded. But it is not by such means that the boundaries of science are extended.

"Among the manifest advantages of a copious and definite nomenclature, may be mentioned, in the first place, the necessity which it demands of an accurate investigation of phenomena, or, in other words, the habitual analytic turn which it tends to give to our inquiries, and therefore the general improvement of the talent of observation, which it must ultimately produce. Secondly, it contributes to facilitate the means of discrimination, by multiplying, as it were, the instruments of distinct conception; for from a deficiency of terms we are apt to think, and even to observe indistinctly. But, above all, a definite nomenclature supplies us with the means of communicating with precision, the information which we acquire, and therefore contributes directly to the advancement of knowledge, or at least removes an otherwise insurmountable impediment to its progress."

It is singular, that, while the necessity of such a nomenclature in regard to the diseases of the skin could not be overlooked; and while, as objects of our senses, having external characters, the precision so happily introduced into all branches of natural history

history seemed, in this instance, applicable to nosology, that it should have been so long unattempted; and it is still to be regretted, that, although Dr Willan from the very first reduced the nomenclature of elementary cutaneous affections to a state of extreme simplicity and perfection, the vague and inaccurate language of his predecessors is still often employed in the description of diseases of the skin. We therefore think it right to quote, from the commencement of Dr Bateman's work, his definitions of the elementary terms, recommending it to our readers to make themselves thoroughly masters of them, and never to use any other.

Definitions.

"1. PAPULA *(Pimple)*; a very small and acuminated elevation of the cuticle, with an inflamed base, very seldom containing a fluid, or suppurating, and commonly terminating in scurf.

"2. SQUAMA *(Scale)*; a lamina of morbid cuticle, hard, thickened, whitish, and opake. Scales, when they increase into irregular layers, are denominated crusts.

"3. EXANTHEMA *(Rash)*; superficial red patches, variously figured, and diffused irregularly over the body, leaving interstices of a natural colour, and terminating in cuticular exfoliations.

"4. BULLA *(Bleb)*; a large portionof the cuticle detached from the skin by the interposition of a transparent watery fluid.

"5. PUSTULA *(Pustule)*; an elevation of the cuticle, with an inflamed base, containing *pus*.

"Four varieties of pustules are denominated in this arrangement as follows:

"*a. Phlyzacium*; a pustule commonly of a large size, raised on a hard circular base, of a vivid red colour, and succeeded by a thick, hard, dark-coloured scab.

"*b. Psydracium*; a small pustule, often irregularly circumscribed, producing but a slight elevation of the cuticle, and terminating in a laminated scab. Many of the psydracia usually appear together, and become confluent; and, after the discharge of pus, they pour out a thin watery humour, which frequently forms an irregular incrustation.

"*c. Achor*; and

"*d. Favus*. These two pustules are considered by the majority of writers from the Greeks downwards, as varieties of the same genus, differing chiefly in magnitude. The *Achor* may be defined, a small acuminated pustule, containing a straw-coloured matter, which has the appearance and nearly the consistence of strained honey, and succeeded by a thin brown or yellowish scab. The *Favus*, or κηριον is larger than the *achor*, flatter, and not acuminated, and contains a more viscid matter; its base, which is often irregular, is slightly inflamed; and it is succeeded by a yellow, semitransparent, and some-

times

times cellular scab, like a honey-comb; whence it has obtained its name.

"6. VESICULA *(Vesicle);* a small orbicular elevation of the cuticle, containing lymph, which is sometimes clear and colourless, but often opake, and whitish or pearl-coloured. It is succeeded either by scurf, or by a laminated scab.

"7. TUBERCULUM *(Tubercle);* a small, hard, superficial tumor, circumscribed, and permanent, or suppurating partially.

"8. MACULA *(Spot);* a permanent discoloration of some portion of the skin, often with a change of its texture.

"The following terms are used in their ordinary acceptation; viz.

"9. *Wheal;* a rounded or longitudinal elevation of the cuticle, with a white summit, but not permanent, not containing a fluid, nor tending to suppuration.

"10. FURFUR *(Scurf);* small exfoliations of the cuticle, which occur after slight inflammation of the skin, a new cuticle being formed underneath during the exfoliation.

"11. SCAB; a hard substance, covering superficial ulcerations, and formed by a concretion of the fluid discharged from them.

"12. STIGMA; a minute red speck in the skin, without any elevation of the cuticle. When Stigmata coalesce, and assume a dark-red or livid colour, they are termed *Petechiæ.*"

Dr Willan divided cutaneous diseases into eight orders, and founded his classification upon their elementary forms in their incipient stage. But this physician, justly entitled to the appellation of great, on account of his extensive knowledge, and skilful criticism of the works of his predecessors, his unwearied spirit of observation, his precision of language, and his philosophical turn of mind, only lived to complete four of these. The loss we sustained by the death of Dr Willan was considered as almost irreparable. It will be acknowledged to have been an extremely arduous undertaking to pursue the investigation which he had begun; but the research and erudition, and still more the spirit of observation, which prevails throughout the work before us, incontestibly evince that Dr Bateman is a successor worthy of his former colleague. His own labours he introduces to the notice of his readers, with that modesty which is characteristic of real merit.

"To prevent any misapprehension in regard to the nature and object of this volume, it may be necessary to state, that it is not brought forward with any pretensions to supply the deficiencies which have been left in the valuable treatise of Dr Willan, or to be considered as the completion of that original work. Its sole purpose is to present an abstract of the classification proposed by that respected author, together with a concise view of all the genera and species, which he intended that it should comprehend. The materials

rials for the description of the first four orders have been obtained principally from Dr Willan's publication, of which the first part of this synopsis may be regarded as an abridgement: some additional facts, however, have been supplied from subsequent observation. The remainder of the matter has been derived partly from personal experience and research, but principally from a constant intercourse with Dr Willan, upon the subject of these diseases, during a period of ten years, while his colleague at the Public Dispensary, and from his own communications in his last illness, before he departed for Maderia, when he kindly undertook a cursory perusal of his unfinished MSS. for my information, during which I made notes relative to these points with which I was least acquainted. For it was, in fact, his wish, that the profession should possess a sketch of the whole of his arrangement, even when the completion of his own treatise, though distant, was not without hope."

Dr Bateman follows Dr Willan's arrangement almost implicitly, but he is by no means ignorant of the objections to which it is exposed; and again we must take the liberty of quoting from our author, as having expressed our sentiments in a manner which we cannot permit ourselves to alter.

" The imperfections which confessedly belong to it, are not the result of ignorance, inattention, or want of discrimination, but are probably inseparable from the nature of the subject. The truth is, that the various genera of cutaneous disease, as characterized by their external appearances, do not differ in the same essential degree in which the diseases of organs of various structure differ from each other. The same exciting cause will produce different kinds of cutaneous disorder, in different individuals: thus, certain substances, which suddenly derange the organs of digestion, sometimes produce urticaria, sometimes erythema and roseola, and sometimes even lepra and psoriasis; yet each of these shall retain its specific character, and follow its peculiar course: thus also certain external irritants will, in one case, excite the pustules of impetigo, and, in another, the vesicles of eczema. Again, the diseases which commence with one generic character, are liable occasionally to assume another, in the course of their progress:—thus, some of the papular eruptions become scaly, and still more frequently pustular, if their duration be long protracted; the Lichen simplex and circumscriptus, for instance, sometimes pass into psoriasis; the Lichen agrius and Prurigo formicans are occasionally converted into impetigo; and the Prurigo mitis is changed to scabies. Moreover, it frequently happens, that the characteristic forms of eruptive diseases are not pure and unmixed, but with the more predominant appearance there is combined a partial eruption of another character: thus, with the papular strophulus, with the rashes of measles and scarlet fever, and with the pustular impetigo and scabies, there is occasionally an intermixture of lymphatic vesicles. And lastly, the natural progress

of many eruptions is to assume a considerable variety of aspect; so that it is only at some particular period of their course that their character is to be unequivocally decided. Thus, in the commencement of Scabies papuliformis and lymphatica, the eruption is of a vesicular character, although its final tendency is to the pustular form: and, on the contrary, in all the varieties of herpes, the general character of the eruption is purely vesicular: yet, as it advances in its progress, the inclosed lymph of the vesicles acquires a considerble degree of opacity, and might be deemed purulent by cursory observers. In like manner, the original pustular character of some of the forms of porrigo is frequently lost in the accumulating crusts, the confluent ulcerations, and the furfuraceous exfoliations, which ensue, and which conceal its true nature from those who have not seen, and are unacquainted with the whole course of its advancement.

"These circumstances constitute a series of natural impediments to every attempt at a methodical arrangement of cutaneous diseases. But it is more philosophical, as well as practically useful, to compromise these difficulties, by retaining in the same station the different appearances of a disease, in its different stages and circumstances, when our knowledge of the causes and remedies, as well as of the natural progress and termination of it, is sufficient to establish its identity,—than to separate the varying symptoms of the same disorder, and to distribute the *disjecta membra*, not only under different genera, but into different classes of the system, after the manner of Prof. Plenck."

It must also be confessed, that, from the very nature of Dr Willan's classification, it is necessarily artificial, bringing together under the same order diseases nowise allied, and separating diseases strongly resembling each other, and difficultly distinguished. This objection, however, we consider as of little moment; because, the use of an artificial arrangement of diseases, as of plants, or of any branch of natural history, is to assist us in recognizing the individual species which is the object of our research; and we shall often find that the less natural, as it is called, the orders are, the more easy does our task become; and it does not in the least retard our investigation of their analogies and diagnosis, that they are not arranged in immediate succession, but in distinct orders.

But the arrangement is Dr Willan's, and it is Dr Bateman who is at present before our tribunal; and truly few have been in that situation who have merited such unqualified approbation. His mode of treating his subject, renders his work a perfect model for works of this description. To every subject a due degree of importance is attached. On each, nothing superfluous seems to be said, while nothing necessary is omitted. The description, aetiology, prognosis, diagnosis, and method of cure, are all detailed

tailed in the most perspicuous and satisfactory manner; and wherever he has thought proper to enter into any literary or historical details, his attainments as a scholar are no less conspicuous than his merits as a physician. To this general encomium upon the work, it is scarcely necessary to add our opinion, that it ought to be in the library of every member of the profession. Its nature totally precludes the possibility of any analysis or condensed view of its contents, and it would be nugatory to select parts, either because they pleased us particularly, or suggested some frivolous objection. In this dilemma, we have chosen, as a short and interesting specimen of the style of the work, our author's observations on the use of cold in scarlatina.

"We are possessed of no physical agent, as far as my experience has taught me, (not excepting even the use of blood-letting in acute inflammation,) by which the functions of the animal economy are controlled with so much certainty, safety, and promptitude, as by the application of cold water to the skin, under the augmented heat of scarlatina, and of some other fevers. This expedient combines in itself all the medicinal properties which are indicated in this state of disease, and which we should scarcely *a priori* expect it to possess; for it is not only the most effectual *febrifuge*, (the "febrifugum magnum," as a reverend author long ago called it *;) but is, in fact, the only *sudorific* and *anodyne*, which will not disappoint the expectation of the practitioner under these circumstances. I have had the satisfaction, in numerous instances, of witnessing the immediate improvement of the symptoms, and the rapid change in the countenance of the patient, produced by washing the skin. Invariably, in the course of a few minutes, the pulse has been diminished in frequency, the thirst has abated, the tongue has become moist, a general free perspiration has broken forth, the skin has become soft and cool, and the eyes have brightened; and these indications of relief have been speedily followed by a calm and refreshing sleep. In all these respects, the condition of the patient presented a complete contrast to that which preceded the cold washing; and his languor was exchanged for a considerable share of vigour. The morbid heat, it is true, when thus removed, is liable to return, and with it the distressing symptoms; but a repetition of the remedy is followed by the same beneficial effects as at first†.

"Partly

* "Dr Hancocke, rector of St Margaret's, Lothbury, published a pamphlet in 1722, entitled "*Febrifugum Magnum*; or, Common Water the best cure for all Fevers, &c." which contains many sound observations and valuable facts, detailed in the quaint language of the time."

† "After the extensive evidence, which a period of more than twenty years has furnished, in proof of the uniform efficacy and security of the external use of cold water, in Scarlatina, and in other febrile diseases connected with high morbid

"Partly from the difficulty of managing the cold affusion, and partly from its formidable character in the estimation of mothers and nurses, imbued with the old prejudices, I have generally contented myself with recommending the *washing* of the skin with cold water, or water and vinegar, more or less frequently and extensively, according to the urgency of the heat. In the beginning of the disease, the affusion of a vessel of cold water over the naked body is, doubtless, the most efficacious: but, by a little management, all the benefits of a reduction of the morbid temperature, that can be expected at a subsequent period, may be obtained by the simple washing. In less violent cases, washing the hands and arms, or the face and neck, is of material advantage*.

"It is, of course, necessary to enjoin the cool regimen, as directed for the Scarlatina simplex; to attend to the ventilation and moderate temperature of the apartment, and to administer the drink cold†".

These excellent observations, the result of experience, are sufficient to impart to the most timid practitioner confidence in the use of cold washing and drink in Scarlatina simplex and anginosa, where the skin is *hot and dry*; but we wish that our author had been more particular in regard to the treatment of scarlatina maligna, where the cold or tepid affusion is inadmissible, on account of the temperature of the skin being below rather than above the usual standard. In general all remedies fail, and the patient dies on the second or third day.

IV.

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morbid heat of the skin, it is to be lamented that some practitioners still look upon the practice as an *experiment*, and repeat the remnants of exploded hypotheses, about repelling morbid matter, stopping pores, &c. as reasons for resisting the testimony of some of the greatest ornaments of the medical profession. For my own part, I have been in the constant habit of resorting to the practice at every opportunity, in Scarlatina, (and also in typhoid fevers, during my superintendence of the Fever Institution for the last nine years,) attending to the simple rules laid down by Dr Currie, and I have never witnessed any inconvenience, much less any injury from it, but an uniformity in its beneficial operation, of which no other physical expedient, with which I am acquainted, affords an example.

* "For the direction of those who may not be acquainted with the principles of this practice, if any such remain in the profession, it may be stated, in the words of Dr Currie, that the cold washing is invariably safe and beneficial, "when the heat of the body is steadily above the natural temperature,—when there is no sense of chilliness present,—and no general or profuse perspiration." But I have found the following direction to the nurses amply sufficient; viz. to apply it, "whenever the skin is *hot* and *dry*." Dr Stanger, in treating Scarlatina among the children of the Foundling Hospital, found no other precaution necessary. "Its effects in cooling the skin, diminishing the frequency of the pulse, abating thirst, and disposing to sleep, were very remarkable. Finding this application so highly beneficial," he adds, "I employed it at every period of the fever, provided the skin were hot and dry." See a *note* in Dr Willan's Treatise, p. 360.

† "Cold drink is, like the washing, always salutary in the same hot and dry state of the skin, and tends, like it, to promote perspiration."

IV.

Report of the National Vaccine Establishment, dated 22d April 1813. pp. 17, Folio.

Rapport du Comité Central de Vaccine, sur les Vaccinations pratiquées en France, pendant l'annee 1810. pp. 143. 8vo. à Paris, 1812.

WHY is it, that the advantages expected from vaccination have been less realized in Great Britain in general, and in London in particular, than in any other place into which it has been introduced? Why have they ceased to be progressive so long before they have reached their maximum?

The reports before us, in some measure, enable us to answer these questions, by referring the difference to the nature of the public measures recommended and enforced by the various legislatures. The British Parliament rewarded Dr Jenner, and they erected a National Vaccine Establishment, to which they delegated the responsibility of propagating vaccination. Now, the manner in which this trust is fulfilled is fundamentally erroneous. There is no arrangement of measures for combining the agency of the profession at large, so as to attain, most certainly, the great object of the institution. A meagre annual report is published, stating, in very general terms, the antivariolous efficacy of vaccination, and informing us of its progress in various British colonies, and other foreign countries, and also recommending surgeons not to inoculate with small-pox virus. But of professional information on the subject these reports are lamentably barren. No new facts relative to the history of the disease are promulgated, to add to the knowledge of believers, or calculated to convict infidels—no anomalous or adverse occurrences are detailed—no subjects of speculative or practical inquiry are suggested—no plan for promoting general vaccination is recommended—no encouragement is held out to the exertion of individuals. On the other hand, we must not overlook what they do, in regard to London. They inform Lord Sidmouth, "that, during the year 1812, the surgeons appointed by their authority to the new station in London, have vaccinated 4,521 persons, and have distributed 23219 charges of vaccine lymph to the public." Such are the labours and expences of the board, in return for an annual grant of L. 3000; and they very gravely tell us, that "they have again the pleasure of stating, that the money granted by Parliament during the last session, has been sufficient to defray the expences of the year 1812, and they are of opinion, that the same sum will be adequate to the expenditure of the current year."

The

The French government proceeds upon different principles. It endeavours to obtain the co-operation of the whole profession, by the institution of a central committee of vaccination at Paris, under the immediate superintendence of the Minister of the Interior, and by issuing a decree to the Prefects of all the departments, enjoining the establishment of a committee in each capital, and requiring a regular return of the number vaccinated, &c. It also excites the emulation of the members of the profession, by publicly conferring premiums and honorary rewards on those who have vaccinated the greatest number, and by recording and giving due credit to the observations of each individual, in the annual report, drawn up from the accumulated communications of all, with great care and accuracy, by the central committee. In short, there is a general plan for obtaining the umost assistance from every individual of the profession, throughout the French empire, for the great purposes of promoting vaccination, and elucidating every inquiry connected with it.

The reports themselves are not mere matters of form, but highly interesting and instructive documents, by the minuteness of their details, not only regarding vaccination, but on every question connected with it. The very solemn and ceremonious manner in which the report is received, at a meeting convened on purpose, and numerously attended by ministers, members of the Institute, the professors of medicine, physicians, surgeons, and students, is calculated to evince the importance attached to vaccination by the government. But to proceed to the matter of the report before us.

It is divided into two parts. The first contains an account of all the administrative measures adopted in the different departments, to ensure the success of vaccination; the second contains the proofs of the regularity, safety, and advantages of vaccination, forming an abstract of the observations of the committee, and of all the professional men of the empire. Our limits will not permit us to make a regular analysis of the details, or to extract all the interesting facts recorded. We shall select one or two points which appear to us peculiarly interesting. In choosing the first, we were influenced by an opinion lately promulgated by Dr Watt of Glasgow, which the French report materially assists in refuting. The effect of the general introduction of vaccination upon population, was grossly overrated by the ignorant; and we have seen, in some of our philosophical journals, calculations of the numbers it would add, in a given term of years, to the inhabitants of these islands, which violated every principle of political arithmetic. The most correct ideas upon that subject, have been promulgated by M. Duvillard, in a most profound and scientific treatise, written expressly

pressly to investigate it *. The following tables shew what would be the increase of the population of France, 134 years after the universal introduction of vaccination, from that cause alone, and supposing new sources of prosperity admitted of such an increase. The period of 134 years is chosen, because it appeared that 24 was the medium age at which the French people married, and 110 seemed to be the extreme term of human life, and therefore the relation of births and deaths at each age would not be regular, until 134 years had elapsed.

Ages	Absolute addition to those living at each of the annexed ages.	Relative addition per cent.	Absolute addition to the population above each of the annexed ages.	Relative addition per cent.
0	0.723.356	72.34	19.587.008	68.01
10	0.460.144	88.49	14.260.397	63.42
20	0.382.637	76.19	10.211.473	59.35
30	0.300.430	68.56	6.796.954	54.39
40	0.224.153	60.68	4.181.933	49.45
50	0.157.325	52.96	2.281.452	44.57
60	0.097.336	45.58	1.013.012	39.68
70	0.045.354	38.55	0.309.058	34.64
80	0.011.057	31.86	0.046.601	29.21
90	0.000.976	25.48	0.003.485	23.54
100	0.000.040	19.34	0.000.079	18.33

In the preceding table, the natural population is supposed to be 28.760.192, which, in 134 years, would become 48.350.200, giving 19.587.008 as the addition at that period, in consequence of universal vaccination; or an increase of the population in the proportion of 1.680.975 to 1.000.000, and the births in the 135th year would be as 1.723.356 to 1.000.000.

But all these calculations proceed upon the principles, that small-pox has no other influence upon population, than by the number of deaths it directly occasions, and that it does not affect in any way the future health or longevity of those who pass through it. And certainly we might naturally suppose, that the beneficial influence of vaccination upon population would even exceed the calculation, because it is consistent with observation,

* Analyse et Tableaux de l'influence de la petite virole sur la mortalité à chaque age, et de celle qu'un preservatif tel que la vaccine peut avoir sur la population et la longevité. Par E. E. Duvillard (du Léman) pp. 210 4to, à Paris, 1806.

tion, that small-pox excites latent diseases into action, while no such bad effect follows vaccination. But a very different conclusion is drawn by Dr Watt from his examination of the bills of mortality of Glasgow for the last thirty years, which he has condensed in the following summary, divided into five equal periods of six years each. The first three of these had passed before the vaccine inoculation could have had any influence; in the fourth, it had nearly reached its maximum, and in the last, it was pretty fully established.

Periods.	Small-Pox.	Measles.	Chincough.	Stopping.	W. in Head.	Teething.	Bowelhive.	Still-born.	Fevers.	Under Two.	Under Five.	Under Ten.	Total under Ten.	Total deaths in the Registers.
I.	19.55	.93	4.51	2.54	.74	4.	6.72	5.03	12.65	39.40	10.66	3.42	53.48	9994
II.	18.29	1.17	5.13	3.33	1.73	4.12	6.43	5.53	8.43	42.38	11.90	3.79	58.07	11103
III.	18.70	2.10	5.36	2.47	2.14	2.41	6.47	6.33	8.24	38.82	12.21	3.45	54.48	9991
IV.	8.90	3.92	6.12	4.93	2.11	1.89	7.27	6.69	9.87	33.50	13.45	5.10	52.03	10034
V.	3.90	10.76	5.57	5.18	2.45	2.24	9.26	6.70	6.49	35.89	14.22	5.58	55.69	13354

Now, on examining these tables, we first observe, what we naturally expected, a great diminution of the deaths from small-pox, from 19.55 per cent. of the total deaths of the register, to 3.90; but, upon looking at the total deaths under ten years of age, we are mortified to find, that there is no corresponding decrease, but on the contrary, that the beneficial influence of vaccination has been totally annihilated; and, according to the tables, this is chiefly owing to the astonishing increase of the mortality of measles. In short, "in the small-pox we have the deaths reduced to nearly a fifth of what they were twenty-five years ago, and in the same period, the deaths by measles have increased more than eleven times." Now, waving any objection that might be made to the accuracy of the number of deaths by measles, is the increased mortality of the latter at all connected with the introduction of vaccination? Dr Watt thinks it is, and advances a hypothesis to account for it.

"But however novel and strange the opinion may appear, it must be admitted, that, while the small-pox were in full force, they had the power of modifying and rendering the measles mild; and now that they are in a great measure expelled, the measles are gradually coming to occupy the same ground which they formerly occupied. am sorry to make this statement, but the facts, at least with regard to Glasgow, are too strong to admit of doubt."

Without

Without inquiring into the validity of the reasoning upon which the hypothesis is hazarded, we are not at all satisfied that the connection which it is intended to explain is established. For both the period of time is too limited in extent, and even the populous city of Glasgow too narrow a field, to give great confidence to the conclusion, unless corroborated by observations made in other places. During the first period, Glasgow may have been accidentally unusually exempt from measles; hence there would be a greater number of subjects liable to the contagion in the subsequent periods. This idea is corroborated by observing, that during the third period, the number of deaths from small-pox is increased, as well as that from measles, which is contradictory of the connection supposed by Dr Watt; and during the last period, some uncommonly fatal epidemic of measles may have prevailed, totally unconnected with the influence of small-pox. Moreover, local circumstances existing in Glasgow, gradually increasing in their influence, may have a direct influence on the increased mortality from measles.

This appears the more probable, because, according to Sussmilch's tables, the average mortality under 10 years is 43 *p. c.* for the country, 47.7 for small towns, and 50.2 for large cities,—even in London itself, it is but 48.4; and, according to Duvillard's calculations, the total deaths in France below 10 are only 44.89. It is therefore proved, that the mortality of children in Glasgow, being 54.75, is excessive, from whatever cause it may proceed.

Next, as to the mortality by small-pox before the introduction of vaccination, Dr Watt gives 18.82 *p. c.* as the average of Glasgow from 1783 to 1800, whereas in London, from 1700 to 1800, it is only 8.58; at Geneva, from 1580 to 1760, 5.77; at Edinburgh and Leith, from 1743 to 1763, 10.38; at Berlin, from 1757 to 1774, 8.23; at Leipsic, from 1758 to 1764, 4.19; at Vienna, 8.; at Breslau, from 1721 to 1724, 9.41; at Lebin, from 1765 to 1775, 11.3; at Salzwedel, from 1765 to 1774, 17.85; in 140 villages in Germany, from 1765 to 1774, 15.13:—so that the mortality at Glasgow is very nearly double the average of all the places mentioned, which is only 9.884.

With regard to the mortality by measles we have fewer data. The average of Glasgow, from 1783 to 1800, is 1.8 *p. c.*; of the total deaths in London, from 1700 to 1800, it is 0.877; at Berlin, an average of 17 years, 1.31; at Salzwedel of nine years, 1.3; at Lebin of 20 years, 3.5; in 140 German villages, from 1766 to 1774, 2.2. These facts only shew that the mortality of measles is very variable, and that, in no other instance upon record, is at all comparable to that in Glasgow during the last six

years. But, as this is the only record of deaths by measles since the general introduction of vaccination, we have still to inquire, whether they be in any way connected? We well remember, that in spring 1808 the measles were fatal to an unprecedented degree in Edinburgh, and popular clamour ascribed their mortality to the introduction of vaccination; but this prejudice was completely removed by the circumstance of the measles being particularly fatal among a body of men, the Inverness Militia, then in Edinburgh, of whom probably few had been vaccinated, and the greatest part must have had the small-pox in its natural form. This fact alone is conclusive as to measles being sometimes unusually fatal, where it is impossible vaccination could have had any influence. We shall next endeavour to shew that measles have not been more fatal than usual where vaccination has been generally introduced. Unluckily we have no access to any bills of mortality during the proper period, but, so far as our experience goes, since the epidemic of 1808, the measles have not been more than usually fatal. We have the authority of a respectable practitioner to make a similar statement in regard to London, and it is with extreme pleasure that we are enabled to quote the following passage of a letter from Dr Bevan of Monmouth.

"Never, in the whole medical history of our parish, have hooping-cough and measles been so prevalent as within the last ten months, and never was their progress marked by fewer instances of serious disease. We have had a very sickly year. In addition to the diseases above mentioned, violent pneumonic affections prevailed among infants in spring, and an unusual number of adults were attacked with idiopathic fever, and yet without much increase in the average number of deaths, as appeared from the examination of the parish registers for 30 years. I made myself acquainted with the causes of the death of all the children under 10 years of age, (which composed more than 2-5ths of the whole deaths) who had been buried at Monmouth in 1813. I found that only *two* had died of hooping-cough, *one* decidedly of measles, and *one* in about a fortnight after measles, so that the death may be probably considered as the consequence of that disease. Now, it is my firm belief, that, in the period between the 1st of January and the 1st of November 1813, not fewer than 150 persons have gone through the measles in this parish, and from 80 to 100 have recovered from chincough. If you take in the neighbourhood of our town, the number of cases of measles would certainly be above 200, and I cannot, after pretty accurate inquiry, hear of a single death besides those I have given above. It is worth noticing, that the death from measles which is positive, occurred in a child of eleven months old,

old, whom I saw on the fourth day of the disease, in the mother's arms, supported literally *over* the fire, and liberally supplied with *cider* and *sack-whey:* the child died on the sixth day, and, you will agree with me in thinking, not from the disease, but from the treatment."

Lastly, in regard to the conclusion drawn from the Glasgow bills of mortality, that vaccination has had no effect in diminishing the deaths under 10 years of age, or of increasing the population, Dr Heberden, junior, has, in the first place, furnished us, as will be seen in a subsequent article in this number, (See p. 110.) with an important document in regard to the decrease of deaths under ten years in London; and we have, in the French vaccine report before us, to which we now return, after this long but pertinent digression, the most satisfactory statements. M. Blanche, surgeon at Rouen, examined the registers there. During the eight years preceding vaccination the deaths were 24.519; during the eight succeeding it, they were 20.165, being a diminution of 4.354 deaths, or of 500 annually. M. Barrey states, that at Bensançon there were from 1793 to 1801 inclusively, 9.320 births, and 9.706 deaths, and therefore a diminution of population of 386; but, from 1802 to 1810 inclusively, 9.122 births, and 8.087 deaths, therefore an increase of 1.035, and 1.619 fewer deaths in the nine latter than in the nine former years. "It further appears from a very accurate abstract of the bills of mortality of Bensançon, that this difference has taken place solely in the deaths which have happened during the *first ten years*, that is, during the period when the small-pox commonly proves most fatal."

At Toulon vaccination was generally introduced in 1806, and the following table by M. Blain, shews the progress of the number of deaths under fifteen years of age since that time:—

Years,	1806,	1807,	1808,	1809,	1810.
Deaths,	2.211,	1.718,	1.571,	1.455	1.300.

At Aigurande, in 1796, there were 1600 inhabitants; at the beginning of 1811 there were 1914; from 1796 to the end of 1808, 196 had died of small-pox. Vaccination was then introduced, and during the next three years the births averaged 80.7, the deaths 71.7, and the deaths by small-pox amounted to 21; vaccination then became general, and, during the next four years the births averaged 78, the deaths 52, and no deaths from small-pox occurred.

At Creutznach Dr Jung states, that, of 13.027 inhabitants of the canton, 293 died in 1808, or $\frac{1}{44}$; of 13.433 in 1809, 260, or $\frac{1}{49}$; and of 14.237 in 1810, 282, or $\frac{1}{50}$; and he adds, that,

from the increased number of children, it has become necessary to enlarge the schools.

At Borgo San Donino several hundreds used to die annually of small-pox. The following table exhibits the facts of some of the last years :—

	Births.	Deaths.	Vaccinations.	Deaths by Small-pox.	Excess of births.
1807.	not known.	not known.	10.150	74	not known.
1808	4.759	4.508	3.439	4	251
1809.	5.519	4.180	3.874	0	1.339
1810.	5.845	4.588	3.520	0	1.257

M. Petiet, physician at Gray, states, that, since vaccination has been introduced into that circle, the deaths *under five years* have annually been sensibly lessened.

In the department of the Arno a murderous small-pox was epidemic in 1809. The prefect, M. Fauchet, took the strongest measures; he prohibited small-pox inoculation, and formed a vaccine committee in every canton. The result was,

1809, 2.450 vaccinations, 5.258 cases of small-pox, 3.400 deaths.
1810, 10.291 ditto, 366 ditto, 40 ditto.

Lastly, the following table shews the results in the department of Dordogne :—

	Births.	Deaths.	Vaccinations.	Excess of Births.
1806.	14.383	11.281	2.250	3.102
1807.	15.478	10.851	3.501	4.627
1808.	15.568	10.236	2.963	5.332
1809.	15.600	11.103	4.323	4.497
1810.	13.810	9.351	8.920	4.449

These facts sufficiently prove that the unexpected conclusion from the Glasgow tables depends upon some local or temporary peculiarity, and that the very reverse, so far as relates to population, and the deaths below ten years of age, is generally true in France; and we confidently expect that the next report of the National Vaccine Establishment will furnish as satisfactory a refutation in regard to Britain.

The only other point mentioned in the French report which we shall notice, is the experimental inquiry in regard to the alleged

alleged power of the cow-pock in preventing the sheep-pock, *elaveau*. The sheep-pock appeared on five sheep of a flock of 233 sheep. All the others were immediately and carefully vaccinated. In six or seven days, on 119 of these, two or three pustules appeared where the puncture had been made, and, some days afterwards, the sheep-pock appeared upon those sheep with whom the vaccination had failed, so that the efficacy of the operation obtained credit. But about the forty-fifth or forty-eighth day after vaccination, those in whom it produced pustules were also attacked with the sheep-pock; they suffered as severely from it as the others, and some even died. In another instance, some sheep which had been vaccinated took the sheep-pock by contagion, while others which had been inoculated with the sheep-pock (clavelized) resisted the contagion, and therefore this latter operation seems preferable, especially as it is not attended with danger, not one having died out of 200 on which it was performed.

V.

Some Account of an Uncommon Appearance in the Flesh of a Sheep; with Reflections on the Nutrition of Sheep, &c. By WALTER VAUGHAN, M. D. Licentiate of the Royal College of Physicians of London. London, 1813, pp. 38. 8vo.

THE subject of this book is a *mutton chop*; and no one could come more opportunely to satisfy the cravings of hungry critics. But it is necessary to know the nature of the chop, to conceive an idea of the feast set before us. To be serious: "In December 1812, as the writer of the following pages was walking through Strood, he saw, in a butcher's shop, some mutton, which, as his mind was then unoccupied with other things, made a deep impression upon it." The flesh, which should have been red, "appeared to him to be white." "He attended to it for some time with no small curiosity." Nay he proceeded the length of "confirming his observation by an experiment." He "even scraped" the mutton which had made such an impression on his vacant mind. The butcher was not at home, but he did not lose the opportunity of selling some of his

unsound mutton by his negligence, for our author "never thought of purchasing any of the mutton," but "he would have made many inquiries concerning it." He meant to return after a few hours, "not suspecting that the appearance of the mutton would recommend it to the most ignorant as a delicacy." But he was mistaken; and "he had nearly lost for ever the advantage which he now enjoys, of possessing a part of the mutton;" his good fortune, however, did not desert him, "for, while he was sitting in the midst of his family, and relating what he had seen, he received a present of chops, cut from the very mutton he was describing."

The doctor was transported,—all his energies were exerted,—and these produced the pamphlet before us, which, for order's sake, he divides into three parts: 1. Observations and experiments; 2. Inquiries; 3. Reflections.

The observation, and to preserve it is the only good reason we can give for noticing such a publication, is that, in the chops presented to our author, both cervical and lumbar, the muscular parts seem to have been completely changed in their nature. Their bulk was not altered, but their substance resembled in colour, texture, and consistence, the fat usually found about the kidneys: even the compound microscope could discern no vestige of fibre, but only minute particles, the figure of which it was impossible to ascertain, and they nowhere exhibited striæ, which could be conceived to be the remains of blood-vessels. This change was greatest in the muscles of locomotion, most frequently and forcibly put in action, and greater in the lumbar then in the cervical chop, and seemed to have begun, our author tell us, in those most distant from the heart. The change also extended to the tendons, ligaments, and cartilages, for the bone lies in close contact with the substance resembling fat. The proper fat of this animal seemed to have undergone no change. We come now to the author's experiments. Hung up for a fortnight in a larder, his chops smelt as sweet as ever. Writing-paper pressed upon the muscular substance was rendered greasy and impervious to water. The next three experiments, being the most important, and not free from ambiguity, we shall quote in his own words.

"5. Plunged into water, while it was boiling, the substance manifested no contraction or increase of density; and when boiled in the water for some minutes, no albuminous scum rose to its surface.

"6. Cut into pieces, separated as much as possible from any thing like membrane, and melted with water till this was entirely evaporated, the substance was rendered whiter and freer from both the smell and the taste of mutton.

"7.

" 7. Prepared in this manner (9), and melted slowly, Fahrenheit's thermometer immersed in it, as soon as a pellicle appeared on its surface, rose to 107°, and sunk to 100° as soon as it was all congealed. Its combustion was like that of hard suet, but more noisy."

Now, if it was so fusible, that, if it did not congeal until the thermometer sunk to 107°, why did it not melt instantly when plunged in boiling water, instead of exhibiting the phenomena he describes in No. 5. As we conceive it to be an interesting pathological question to ascertain, whether the muscular fibre of a living animal was actually ever converted into real fat, we would recommend, that a deputation of the society for promoting the study of animal chemistry, should lose no time in proceeding to the shop of John Harding, 33, St James Street, to examine the chop, which is left there for the inspection of the curious.

Dr Vaughan's inquiries gained him the following information. It was a wether, two years old, bred in Romney-marsh, fed on nothing but grass, as large as other sheep of the same age, with nothing peculiar in its wool, and had never laboured under any disease, until a fortnight before it was killed, to preserve it from dying. It grew so weak, especially in its hind-quarters, as to totter and frequently to fall down. On cutting up, the fat of the mesentery, kidneys, &c. was deficient in quantity, but the blood of the animal coagulated, and the liver, heart, and lungs, appeared sound. We wish he had inquired at his friend, Mr T. E. Hulkes, so precise and perspicuous in his answers, whether he had ever observed any thing similar before.

We shall not attempt to follow Dr Vaughan through his third section, which treats *de omnibus rebus et quibusdam aliis*, but only assure him, that he is in the right, in supposing that " his thoughts, however desultory, could give no offence, and much less provoke resentment." On the contrary, we return him thanks for the communication of the pathological fact, if, indeed, he be not altogether mistaken,—and for the amusement he has afforded us, by the manner in which it is communicated.

VI.

Nouvelles Expériences Galvaniques, faites sur les organes musculaires de l'Homme et des Animaux à Sang Rouge; dans lesquelles, en classant ces divers organes sous le rapport de la durée de leur excitabilité Galvanique, on prouve que le cœur est celui qui conserve le plus long-temps cette propriété. Par P. H. Nysten, Médicin, Membre associé de la Société des Observateurs de l'Homme. A Paris, An 11. pp. 144.

The object of this work is to ascertain the comparative duration of the excitability by Galvanism of different organs in animals of different kinds, and killed in different manners. He divides it into three parts: The first contains experiments made on the body of a man who was guillotined; the second on animals with red and warm blood; and the third on animals with red and cold blood. The difficulties and even dangers with which the first experiment was attended, prove the zeal with which M. Nysten prosecutes his studies; but at the same time could not but be disadvantageous to his investigation. M. Nysten was not able to get the thorax opened until 40 minutes, nor the abdomen until 60 minutes after the decapitation of the criminal. The œsophagus, stomach, intestines, bladder, aorta, and left ventricle of the heart, although still very warm, were insensible both to mechanical and Galvanic stimuli. But the excitability of the right ventricle did not cease entirely until one hour and 48 minutes; that of the voluntary muscles until 4 hours and 11 minutes; nor that of the right and left auricles until 4 hours and 41 minutes after death. From the whole of the experiment, M. Nysten draws the following conclusions; and, so far as one experiment performed in very unfavourable circumstances can warrant, correctly.

1. The galvanic action supports the excitability of the heart, and even renews it when about to cease. This was proved by the singular fact, that the right auricle of the heart did not shew any remains of excitability, either to mechanical or Galvanic stimulus, for an hour and three quarters after death; but when at last excited by Galvanism, it not only continued excitable by it for a long time, but became even sensible to mechanical stimuli. Also, when the hearts of dogs have been left for a considerable time at rest, it is often difficult to renew their contraction, but when once excited by repeated applications of Galvanism, their excitability becomes more and more marked. These facts also shew, that, upon some occasions, the excitability of organs may be supposed to have ceased, when in fact it was only dormant; and

and that experiments in which positive effects have been produced, are not to be controverted by others which only warrant negative conclusions.

2. The extreme frequency of Galvanic excitations diminishes temporarily the excitement of the heart.

3. The Galvanic excitability of the heart continues after the apparent extinction of animal heat.

4. The different parts of the heart lose their Galvanic excitability in the same order in which they lose their mechanical excitability; first the left ventricle, then the right, next the left auricle, and lastly the right.

From fifteen experiments related in the second part, M. Nysten concludes:

1. That the relative duration of the excitability of the heart, is the same in the mammalia and in birds, but that the absolute duration is greater in the former than in the latter.

2. That the different kinds of violent death produced by means purely mechanical, have little influence on the Galvanic excitability.

3. That the temperature of the atmosphere seemed to have some influence on the duration of the excitability in birds, but very little in dogs, and that only in the voluntary muscles.

4. That the scale of the duration of the Galvanic excitability in the mammalia varies according to the species, but in all, the heart occupies the top of the scale. In dogs the following order was observed.

The heart continues excitable from 5 hours and 10 minutes, to 8 hours and 33 minutes after death.

The œsophagus from 1 hour and 21 minutes, to 3 hours and 40 minutes.

The voluntary muscles from 47 minutes, to 2 hours and 20 minutes.

The bladder from 45 minutes to 1 hour.

The small intestines, from 25 minutes to 1 hour.

The large intestines, from 15 minutes to 30 minutes.

The aorta and the womb were totally inexcitable by Galvanism.

It is also deserving of notice, that, when death is induced by strangulation, the heart, instead of being the last, is the first organ to lose its excitability. This remarkable phenomenon M. Nysten ascribes to a physical hindrance produced by the excessive distention of that organ with blood which takes place in that case; because, if immediately after the production of asphyxia, the systemic venous trunks be opened, the heart recovers its excitability as soon as the distention is removed, and if this distention be induced by any other kind of death, an electric shock

VII.

A Treatise on the art of Cupping, in which the history of that operation is traced; the various diseases in which it is useful, indicated; and the most approved method of performing it described. By THOMAS MAPLESON, Cupper of his Royal Highness, the Prince Regent; to the Westminster Hospital, &c. London, 1813. 3s. 6d.

IT has been too much the practice of medical men, to limit their more attentive and arduous researches to those objects where an exhibition of their talents shall be the most distinguished. Thus both diseases, and the operations necessary for their relief, which are not of a fatal tendency, or which do not impair the functions of any of the vital organs, have not met with the due consideration they merit. There are many teazing and molesting ailments for which the science of medicine affords no relief, and there are many of the less operations of surgery which able and experienced men perform clumsily and unsuccesfully. We therefore consider, that the author of the present treatise is entitled to much praise, for having pointed out the means by which a very useful operation may be performed with ease and success; an operation which has usually been considered as extremely difficult, and whose principles have generally been very ill understood.

"The first step in the operation of cupping, is to produce a partial vacuum over one or more portions of the surface of the skin. This purpose, as has already been stated, was formerly effected by sucking the air out of a hollow gourd or cucurbit, by the application of the mouth, or expelling it from a similarly shaped metallic vessel, by the introduction of a piece of inflamed flax. The invention of glass has wholly superseded these contrivances, and small bell-shaped glass cups are now universally used for the purposes of exhaustion, as being neat, cleanly, light, permitting the colour and quantity of blood to be judged of, as it flows from the wounds. For the purpose of exhausting the contained air, these are occasionally fitted up with brass caps and valves, and a small syringe of the same metal. To this plan there are two objections: 1*st*, The exhaustion is apt to be carried too far, which, instead of promoting, tends to obstruct the flow of blood. 2*d*, The operation is rendered extremely tedious and fatiguing, both to the patient and operator, and the glasses are very apt to be broken, by the valves losing their elasticity."

"The

" The air contained in the glass may also be rarified by steam, but the mode which is now universally adopted by regular cuppers, is the momentary introduction of the flame of a spirit lamp, with a thick wick. The larger the glass, when properly exhausted, the less pain does the patient suffer, and (what is of most importance) the more freely does the blood flow.

" When the operation is to be performed, let there be provided a hand-bason with warm water; a piece of fine sponge, and a lighted candle.

" Place as many glasses in the bason as may be requisite to draw the quantity of blood intended to be taken away. If sixteen or twenty ounces be ordered, four glasses, of a size adapted to the surface, will, in most cases, be required. Each glass is then separately to be held, *for an instant*, over the flame of the burning lamp, and immediately placed upon the skin of the patient. Upon the quickness with which this is done, depend the whole neatness and efficacy of the operation. To obviate this want of dexterity, many operators in the country throw a little bit of paper dipped in spirits, and inflamed, into the cupping glass the moment before it is applied—a clumsy expedient, adding unnecessarily to the sufferings of the patient, by cauterizing the skin; doing harm also, by rarifying the air more than necessary within the glass—in consequence of which, the edges of the cup compress the cutaneous vessels so much, as to obstruct the influx of the blood.

" If the glasses have been duly exhausted, the skin will swell gradually within the cup, owing to the pressure of the air upon the parts in the vicinity, as well as the expansion of the fluids contained in the cellular membrane. The skin becomes also of a dark purple colour, owing to the influx of blood into the smaller vessels. If *dry cupping* be only intended, the glasses may be allowed to remain on the skin for a few moments, and replaced five or six times, varying their position a little to prevent bruising the skin. If the intention be to scarify and take away blood, the glass ought not to remain more than a minute, and when it is to be removed, by gently introducing the nail of the fore-finger under the edge, and the scarificator *instantly* applied and discharged upon the skin, before the tumefaction has had time to subside. Upon the rapidity or slowness with which the application of the scarificator succeeds to the removal of the glass, depend all the sufferings of the patient. If the skin has completely subsided before the stroke of the instrument, much unnecessary pain is inflicted.

" The glasses are thus to be removed and reapplied in succession. They should be a second time removed, if necessary, as soon as the blood is perceived to coagulate within them, or when they are so full as to be in danger of dropping off.

" For the sake of neatness, care should be taken to insert the nail under the upper part of the glass, and open it downwards, gently wiping the wounds with a warm sponge. The glass, previous to every application, should be rinsed in warm water, but not dried

To obviate the unpleasant sensation produced by the coldness of the metallic scarificator, it is advisable to pass the instrument, for a moment, over the flame of a lamp, before using it.

"It is a common error to make the incisions too deep, especially when the object is to take away much blood, but I am convinced that nothing is gained by going deeper than the cutis or true skin. The quantity of blood obtained in most cases, depends wholly on the due exhaustion and proper application of the glasses—to attain which requires some practice. In cupping on the back, or neck, the glasses should never be placed upon the spine itself, but on each side of it. If it can possibly be avoided, the glasses should not be applied to parts where the skin is in immediate contact with the subjacent bones; though, if care be taken to apply the scarificator before the tumor caused by the glass has subsided, there is no danger.

"Previously to the application of the glasses, it is of importance to foment the skin well with a sponge and warm water.

"No part of the operation demands more the attention of the operator, than the state of the scarificators. The lancets or cutter should be kept as sharp, and in as fine order as possible, so as to make a clean incision, without bruising or giving much pain."

It is unnecessary to point out the numerous and important advantages to be derived from cupping, as a mode of local blood-letting. In all situations where leeches cannot be procured, more particularly in the army and navy, it is of great consequence that this mode can be at all times commanded, as a safe and certain mode of detracting blood.

We must refer our more inquiring readers to the little work itself, for much useful and interesting information on this subject.

VIII.

Medical Transactions, published by the College of Physicians in London. Volume the Fourth. 8vo. pp. 415. Longman and Co. 1813.

Amidst the important duties which the College of Physicians are bound to fulfil, according to their charter of incorporation,—amidst their arduous labours for the encouragement of medical talent, and for the suppression of every species of quackery,—it must be a subject of gratulation, that they now and then find

find a little leisure, and now and then feel a little zeal, for the cultivation of medical science. These paroxysms of exertion occur, it is true, but seldom and at distant periods, like the visits of the influenza, and their exciting causes are not always cognizable by the multitude. Two hundred and fifty years had passed away, after the incorporation of the college, when the first fit came on; and the first volume of its Transactions was published in 1768. In the course of the ensuing seventeen years, two other volumes were successively produced; and now, after the silent lapse of twenty-eight years more, the fourth volume comes forth, the result of an accumulation of industry of upwards of seven years. The law of nature, indeed, is thus fulfilled, which seems to ordain that, in proportion to the bulk and weight of the body, the periods of conception are rare, the gestation tedious, and the offspring few in number.

As these literary prolusions, are, in fact, works of supererogation on the part of the college, and constitute their recreation from the labour of more solemn duties, it cannot be expected, perhaps, that they should display the same excellence or the same industry, which characterize the productions of some of the voluntary associations of professional men, for the sole purpose of prosecuting pathological inquiry. Hence, although the former volumes of the Medical Transactions contain many excellent dissertations, and especially those of the learned and estimable physician by whose exertions they were principally collected; yet they do not, as a whole, maintain that value in the estimation of the public, which the more original and extensive collections of "Medical Essays" of this place, and of the "Medical Observations and Inquiries" of London, possess. If, however, this opinion of the comparative merits of the earlier publications of the college be deemed questionable, we are persuaded that the inferior merit of the present volume, either in comparison with preceding or contemporary collections, will not admit of dispute.

The papers, which are now first published, are twenty-five in number, the volume being concluded by the republication of the Report of the College on the subject of vaccination, which was widely circulated six years ago. The communications are distinguished by very little originality, and bring forward scarcely a single new pathological fact, nor do they serve to establish any doubtful or controverted principle. Generally speaking, they partake either of the marvellous, the common-place, or the trivial, in their character; and these qualities are, in some of them, more or less combined.

The volume opens with a paper, read in April 1806, which bears the respected name of Baillie, from whose pen nothing

trifling can issue. Nevertheless, neither this, nor the two other communications from the same quarter, seem to have been deemed by the author himself of much importance. The first is the relation of a case of a boy, seven years old, in whom an attack of hydrocephalus produced a considerable separation of some of the bones of the cranium, which had previously been firmly united. The rarity of this occurrence induced the author to narrate it, and he considers the case as unique. Perhaps, indeed, there may be no fully authenticated case of this kind on record; but Lieutaud has mentioned two or three instances of such a separation of united bones in hydrocephalus, upon the authority of some older writers*. Dr Baillie's second paper details the symptoms of a hemiplegic attack, in a gentleman of fifty-six; accompanied with a gradual contraction of the palsied limbs, but with none of the usual symptoms of hydrocephalus. The disease proved fatal in eleven months, and a large accumulation of water in the ventricles was the only morbid appearance discovered by dissection. The third paper is more interesting, as containing the experience of this able and extensive observer, respecting a morbid symptom about which little has been written, viz. a strong pulsation in the epigastric region. The causes of this pulsation he admits to be obscure, but believes it to be in most cases connected with indigestion and an irritable habit. His inference from a good deal of experience on this subject is, that "it very rarely depends upon any disease of the aorta itself, or of its large branches in that place; and that this occurrence is almost constantly of very little importance." The improvement of the digestive powers, and above all the removal of the patient's anxiety on the subject, are the only remedial expedients to be adopted.

Dr Latham, who is now, we believe, president of the college, has been the most ample contributor to this volume, having communicated five papers of his own, and a marvellous history, given him by Mr Dampier, the barrister, who received it from an apothecary in Norfolk. The boy, who is the subject of this narration, has most assuredly had a sly dip in the Styx, of which he must have taken a good gulp into his bowels, and, if he have not the heel of Achilles, he may henceforth, inside and out, bid defiance to death and the doctors. He was overturned in a cart, which pressed out, through the anus, "a very large portion of the intestines, with the mesentery," and some of the omentum: yet, notwithstanding this mischief,—notwithstanding the "*spiritu-*

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* See his Hist. Anat. Med. Lib. iii. Sect. 5.

ous fomentation," which was applied to the parts before they were reduced,—notwithstanding the *tinctures of cinchona* and *rhatania* given to allay the *sickness* and *griping*,—notwithstanding that, on the third day, the attendant, on the appearance of gangrenous symptoms, "*cut off the intestine with the mesentery, close to the anus*," to the length of *fifty-seven inches*,—this boy actually recovered, passed his stools per anum, and four months afterwards walked seven miles to dine with his apothecary, and back again the same evening!

Dr Latham's first paper relates to the treatment of tetanus, and is intended to shew "the utility of *relaxant* medicines" in that formidable disease. As a guide to the acceptation in which Dr Latham uses this indefinite word *relaxant*, the pulvis ipecacuanhæ compositus is mentioned as the remedy especially to be recommended; and on perusing the paper, we find that James's powder was the first medicine employed for the purpose of *relaxation*, by which it would seem, therefore, we are to understand *sweating*. After the failure of this antimonial, alone and alternated with opium, in two cases, Dover's powder was given, in a third, in the dose of ten grains every four hours, with an occasional laxative of oleum ricini. At the end of three weeks the patient was free from the spasms. A second case, of a slighter character, was cured in the course of a month, by the same treatment, in Bartholomew's Hospital, and a third in private practice. That the sweating, however, has any thing to do with the cure of this severe convulsive affection is very questionable: the antimonial powder alone did not cure the disease; but opium alone has, in other hands, effected that purpose. In a subsequent part of the volume, we find a "sequel to the paper on Tetanus," which, together with a few common-place observations on the analogy between hydrophobia and tetanus, merely contains a recommendation of the use of Dover's powder in the former, which is advised to be given in the dose of a drachm every two hours by glyster, when deglutition is performed with difficulty. The paper concludes with mentioning the opinion of an anonymous physician, that the "eau medicinale d'Husson" might probably be beneficial in hydrophobia: in which Dr Latham "perfectly agrees with him," and believes that the vinum Veratri, as prepared at Apothecaries' Hall, "would probably be no less efficacious." Perhaps our readers will think with us, that such edifying conjectures were not worthy to be the subject of a serious dissertation; more especially since opiates have been administered in hydrophobia by Dr Vaughan*, Dr Wavel, Dr Babington,

Babington*, and others, to an extent that could only be attained by giving Dover's powder by ounces; yet without any effect. The judicious inference of Dr John Hunter with respect to opium in hydrophobia, after considering the collective experience which medical records afford, was, "that it can only be imputed to want of method and order, that this medicine still continues to be given in almost every case of this disease†."

A paper, by the same author, containing "Remarks on Tumors, which have been occasionally mistaken for diseases of the Liver," is one of very slender materials, consisting chiefly of one or two diagnostic observations relative to induration of the pancreas and to diseased ovarium, which Dr Latham considers as the principal sources of mistake. As far as our observation has gone, the pancreas is less liable to disease than almost any other viscus: for among many dissections, we do not recollect more than one instance in which any morbid change was found in the pancreas. Its healthy, firm, and white appearance has, we believe, sometimes given the idea of scirrhus. Of the proof of the correctness of his diagnostics by dissection, however, Dr Latham says nothing; and the few symptoms, which he points out, are too obvious to dwell upon: viz. the pain on pressure being under the scrobiculus cordis, without any enlargement or tenderness in the right hypochondrium, together with dyspeptic symptoms. And with respect to morbid ovarium, the only circumstance mentioned is, that strong pressure will enable us to discover a sort of chasm *above* the tumor, when it is enlarged ovarium, and *below*, when it is liver.

The last paper of this author, entitled "Observations on certain Symptoms, usually, but not always, denoting Angina Pectoris," seems to make pretensions to some acuteness of diagnosis; but when we have gone through it, we are surprised at the unsatisfactory result at which we have arrived. In the first place, under the appellation Angina, Dr Latham confounds the symptoms of hydrothorax, and of every other organic disease of the chest; and then merely goes on to state, that abdominal tumor, especially hepatic enlargement, may in fact produce, by pressure on the diaphragm, several of the symptoms of pectoral disease,—a truism, of which surely none of his brethren required a demonstration. Mercury, Cheltenham, Lymington, and Bath waters,

* See Medical Records and Researches, Art. 8. Half a drachm of solid opium was given at short intervals, so that 180 grains were taken in eleven hours.

† See Trans. of a Soc. for the Improv. of Med. and Chir. Knowledge, Vol. I. Art. 17.

waters, will of course remove this "Angina *notha*," in many instances. And the following pathetic and discriminative paragraph, will explain Dr Latham's views in less favourable cases.

"Having stated thus much concerning what I have termed the remediable conditions of the disease, allow me to observe, that where morbid organization of the thorax is actually and evidently present, nothing can effect a cure. But must nothing be attempted with a view to alleviation? Must every deplorable circumstance, *each of itself sufficient to cause or constitute the true angina pectoris*, be therefore utterly neglected? Must the aneurismal swelling be carelessly left to expand itself? Must ossification of the heart and its valves, or adhesion of the heart to its capsule, cramping, embarrassing, and confounding its pulsations, remain unheeded? Must habitual obstruction in the lungs, or their condensed and consolidated texture, never be attended to, or the insidious progress of tubercle never be watched or regarded? We may answer, that the more distressing any case shall be, the more earnest ought we to be in our investigation; and although the result may be such as to render it presumptuous in us to expect a cure of the disease, yet it should never prevent us from attempting its alleviation: nay even the kind offices of a friendly physician will often sooth, where his art has absolutely failed him. Let *squills, ammoniacum, assafœtida, camphor, æther, conium, digitalis, opium*, singly or conjointly *with any other remedies* of *regular practice*, be given, as the occasion may require, in those cases where alleviation is only to be expected; but in those *other* cases of *angina pectoris (!)* which I have considered to be sometimes curable, let us not suffer the early opportunity to escape us, but direct our remedies at once to the seat and cause of the disease *boldly*, using mercury, and employing all other appropriate means that may promise relief." pp. 285-7.

Dr Heberden is the next contributor to the volume, and has supplied three papers. The first is a brief sketch of a case of partial *nyctalopia*, or night-blindness, which occurred in a lady, and spontaneously ceased in about five weeks. It was not characterized by actual blindness, but by an indistinctness of vision, during which every object assumed a red hue, and which came on at sunset, and disappeared in the morning. Dr Heberden says that it seemed to have arisen from a strong exposure to light; but he has not mentioned the circumstances. He is disposed to refer all instances of nyctalopia to the influence of continued exposure to strong light, by which the sensibility of the retina is weakened. This may be the case occasionally; but it is clearly, in some cases, connected with dyspepsia, and may be removed by curing the morbid condition of the chylopoietic viscera, as Prof. Scarpa, Richter, and others have stated. Dr Heberden has said nothing relative to the state of the digestive organs in this patient; nor has

has he taken any notice of the circumstance, that the word nyctalopia has been applied by several writers, both ancient and modern, to the opposite condition of the eyes, or day-blindness, from a different view of the etymology of the word.

The second paper of Dr Heberden, though containing nothing novel, is somewhat more important than the preceding: it comprises "some observations on the Scurvy." Although admitting the little reliance to be placed upon the ancient records, in which the term, scurvy, is so extensively misapplied, Dr Heberden believes that the disease was formerly much more prevalent on land, as well as at sea, and that the great diminution of it at present is to be ascribed to the same improvements in our modes of life, especially in respect to cleanliness and ventilation, which have banished the plague, dysentery, and remittent fever. He relates three cases which occurred in St George's Hospital, from which, however, no practical conclusion is to be inferred.

The last paper communicated by Dr Heberden, may be deemed a sort of appendix to the preceding, and to his little work, "on the increase and decrease of different diseases," formerly published: it is entitled "On the Mortality of London." Its principal object is to show the relative increase of the christenings over the burials during the last half century, but especially since the year 1800. "During the first fifty years of the 18th century, the burials exceeded the christenings in the proportion of 3 to 2." "But since the year 1800, the burials have actually fallen short of the christenings in the proportion of 12 to 13." The principal decrease of burials has taken place among children under two years of age, while (as might perhaps be expected, in consequence of the number saved in this early stage of life) the number of those who die between two and ten is much increased. The deaths between the age of ten and forty are nearly in the same proportion now as heretofore; while "in all the subsequent periods of life *till fourscore*, the numbers of the present time are much greater than they were fifty years ago," which Dr Heberden justly ascribes to the increased healthiness of London. He says, "it is remarkable, however, that the proportion of persons, who attain the age of eighty years, has never undergone much variation; as if these extraordinary cases were to be attributed rather to some original conformation of the body, some greater strength of constitution, than to those causes, which influence the preceding course of mortality." This observation seems to be supported by the memorable remark of Lord Mansfield, that of all the aged persons who had come before him, not one had been habituated to the same course of life; the drunk and the sober, the idle and the

the active, had equally attained old age: a circumstance, which is much more satisfactorily explained, we think, upon the principle suggested by Dr Heberden, than by the *early-rising* to which the judge was inclined to impute it.

Dr Powell has related his experience of the internal use of the nitrate of silver in chorea and epilepsy, which he seems to have carried to a greater extent than other practitioners. He is disposed to infer that it is the most effectual of the metallic tonics in the relief of morbid muscular contractions; and he shows that the stomach will bear a much larger quantity with impunity, when it is given in pills, than when swallowed in solution; and that some people are affected by much smaller doses than others can bear; whence an obvious caution that it should be *first* employed in small doses.

In a second paper, Dr Powell details some of the regulations of mad-houses according to the act of Parliament, and points out the obstacles to accuracy in attempting to estimate the proportion of the insane to the sane part of the population; a proportion which he thinks has been overrated. He estimates the aggregate number of insane persons in 1800, comparing it with the census of that year in England, to be in about the ratio of 1 to 7300. He has exhibited the number of lunatics, returned under the act of Parliament in each year, from 1757 to 1809, in a species of chart. It would appear, from this document, that, on the whole, insanity has been on the increase of late years. The fluctuation in the annual numbers, however, is somewhat remarkable. Thus the whole number returned in 1798, was about 430; in 1802, it was 590; in 1807, 415; and in 1809, again about 540. This is a mere *statistical* paper, containing nothing strictly medical; but some imperfections in the act of Parliament are pointed out.

Dr Roberts begins a paper on consumption with the trite observation, that "the ordinary symptoms of phthisis pulmonalis are too well known to require a particular description;" and then he proceeds to fill sixteen pages with a description of symptoms of the disease as trite as this observation. If we do not learn from every vade-mecum and medical pocket-book, that the *expectoration* "is usually of the colour of a *sage leaf*," but that occasionally after hæmoptysis it becomes "of a *fawn colour*, arising from the uniform tinge of blood," we never fail at least to find, that the disease is frequently prevalent in particular families;—that tubercles are the well known cause of phthisis, and are formed in the cellular substance of the lungs;—that in phthisical females the catamenia are commonly suppressed, and that pregnancy often gives a fallacious hope;—that after lingering some

time

time, patients are sometimes suddenly destroyed by the bursting of a vomica,—that hectic fever ultimately appears, " hectic, of which the morning sweat forms the last stage, &c. &c." But if the college could imagine that any of their brethren would be instructed by the publication of the preceding observations, which are estimable in proportion to their antiquity and truth, they might probably conceive that the unique novelty of the concluding remarks, which inform us that a great number of substances will *not* cure consumption, might make the balance even. The author justly boasts of his courage, in publishing the result of his *experiments* (we are obliged, from the poverty of language, to borrow this philosophical term), when he administered for the cure of pulmonary consumption, without pointing out any discrimination of circumstances, the following articles: viz. nitrate of silver, superacetate of lead, the sulphate and oxide of zinc, the arsenical solution, the oxide of manganese, and the black oxide of cobalt, as well as ammoniated copper, muriate of barytes, the nitric and phosphoric acids, and likewise the vegetable narcotics, aconite, hyoscyamus, stramonium, belladonna, and toxicodendron. The result of the long and patient trial of these multifarious drugs, which, the author believes, had not before been exhibited in this complant, is comprised in this laconic conclusion: they " *were administered without success.*"

We turn with pleasure to the relation of " a case of Tetanus arising from a wound, in which the *affusion of cold water* was successfully employed," which is briefly but distinctly related by Dr Currey, of St Thomas's Hospital. The patient was a man of forty, who was admitted into the hospital, under the care of Mr Cline, in consequence of a bruise of the hand by a heavy stone. On the 13th day after the accident he first felt some stiffness about the neck and jaw, but no decided trismus had occurred on the 15th, when Dr Currey first saw him. Four days after this, the jaw had become closely locked, and he had frequent spasms, notwithstanding that he had been taking 36 drops of laudanum, every six hours. He was now exposed naked to the affusion of two pails full of water at the temperature of 62°. He expressed himself considerably relieved on this and every future affusion, which was repeated for twelve successive days, once in every four hours, on the first eight days, and afterwards three times a-day. The opiate was continued during the whole period, and occasional laxative medicines and injections were employed. We had once an opportunity of witnessing the same, although a more speedy, benefit from the use of the cold affusion, in a young man: so decided was the relief he experienced, that he could distinctly mark the progressive relaxation of the jaw by the

the successive affusions. Opium was administered in that instance, nearly in the same quantity as given by Dr Currey; but the patient himself ascribed his relief to the cold affusion.

Dr Warren has contributed two papers to this volume, the first containing an interesting account of "two cases of diabetes mellitus, treated with opium," and the second of headachs arising from indigestion. The two cases of diabetes are fully and satisfactorily detailed, and afford a clear evidence, as far as they go, of the powerful influence of opium in restraining the quantity and moderating or removing the saccharine quality of the urine. In one of the patients, when the opium had been gradually increased to the dose of six grains twice a day, this effect was produced; but the sweetness returned, on the reduction of the dose, and was again removed by increasing it. It is not known, whether this man ultimately recovered. The second patient died, (although the morbid condition of the urine was removed,) of phthisis pulmonalis, and Dr Warren omits to state how soon after this *cure* was performed. It has been observed by some practitioners, that the removal of the urinary disease, by animal diet, opiates, &c. is by no means always equivalent to a cure of the constitutional malady; and that the emaciation, the *tabes*, or consumption connected with it, still goes on to its fatal termination. Was this "rapid" pulmonary consumption only the continuance of the same disease of habit, of which the diabetic discharge had been a symptom? It should be added, that this man was relieved of the diabetic discharge by the enormous dose of five grains of solid opium, taken four times a-day, after a gradual increase.

The paper on the subject of dyspeptic headachs, is the most elaborate essay contained in this volume. The description of particular symptoms is much more minute, than we can generally obtain from patients, and some of those detailed, are such as few patients attend to with sufficient accuracy, to be able to recapitulate them in the way in which they are here described. There is a refinement, too, in attempting to make two species of this headach, the one of which is ascribed to imperfect action of the stomach alone, and the other to a deranged condition of the duodenum, which the observation of disease in others would scarcely lead to; and one of the reasons for this distinction is grounded upon a *sensation*, which involves anatomical knowledge and physiological hypothesis. Patients are said to refer the principal seat of uneasiness, not to the stomach, but "to the upper bowels, where they feel a sensation of *indolence*, to which they attribute their sufferings; and after a very little experience, are aware that some mitigation of the headach will take place as

soon as the *peristaltic motion* of these parts has been quickened, &c." From all these circumstances we are led to conclude, that the learned author himself is subject to these dyspeptic headachs, and that in fact he has given us a description of his own sensations. How far he is entitled to generalize his own symptoms may be questionable. We cannot, for our own parts, clearly recognize the portraits which he has drawn, or at least the small difference which he has attempted to delineate between his two leading varieties of headach, in the cases which we have been accustomed to see. There is considerable ingenuity in the detail, and the practical precepts are judicious and ample. For the first species, which, the author conceives, occurs at a period, "when the mass of food, which has been prepared in the stomach, is passing or has passed into the duodenum, to receive the influence of the bile," a purgative, he says, ought to be given immediately, "and that should be preferred, which has been found to act most speedily upon the stomach and upper bowels of the individual who is the subject of the attack; and the dose of it should be repeated every half hour or oftener, till some mitigation of the pain in the head and mistiness of the sight takes place, or a disposition to action is perceived in the upper bowels." If acidity is present, magnesia is the best and most expeditious purgative, with rhubarb, and saline laxatives, in some cordial water. Calomel he deems objectionable, both on account of the slowness of its operation, and its "mercurial influence." Emetics are of little use, unless they "purge the upper bowels;" and bitter, and other tonic medicines produce little benefit, until the natural habit of evacuation has been in a great measure restored. Headachs arising from imperfect action of the stomach alone, require less active treatment, viz. a slight laxative of aloes, rhubarb, or coloquintida daily, either before dinner, or when experience teaches the patient that he is most likely to suffer. A permanent cure, however, is only to be obtained, by removing the causes of defective digestion. Opiates and other narcotics, combined with caustic potass or magnesia wherever acid is present, are justly recommended as the best means of correcting disordered secretion of the stomach, and relieving the morbid susceptibility of its nerves; and gentle stimulants are judiciously preferred to mere tonics for the purpose of restoring the digestive powers.

The remaining papers, which we have to notice, are of inferior interest compared with those of Dr Warren. As we ascend to the regions of the court, we find that physicians are called upon to prescribe in cases of indisposition so slightly decided, that they are scarcely called disease in lower life, and at all events have not found a name in the ordinary systems of nosology. We

We now, therefore, for the first time, are presented with an account of the "*climacteric disease*" from the pen of Sir Henry Halford. This, as the reader will probably anticipate, is a new nomenclature of what is commonly deemed the natural decay, or breaking up, of the constitution. Sir Henry is disposed to consider it as actually *a disease*, because men frequently rally after it has once appeared, and live many years. He first describes its simple form, which is mere debility, and loss of flesh, with a pulse somewhat quicker than natural, with flying pains, impaired appetite and sleep, and at length anasarca; and then he remarks,

"But it is seldom that we have an opportunity of observing this malady in its simple form, and never, I believe, but in a patient whose previous life has been entirely healthy. We find it generally complicated with other complaints, assuming their character, and accompanying them in their course, and perhaps this may be the reason why we do not find the climacteric disease described in books of nosology as a distinct and particular distemper. It blends itself with the effects of any fixed organic mischief in the constitution; takes on the appearance of any periodical irritation to which a patient may have been subject, or adopts the features of a casual disease. When it is associated with organic mischief, it is difficult to distinguish the climacteric complaint from that train of symptoms which commonly supervenes, sooner or later, on diseased structure; but its presence ought to be suspected, if the complaints are all unusually exasperated, if a fatal result be threatened earlier than usual in the common course of things, and, above all other indications, if that character be impressed on the countenance which peculiarly distinguishes this disorder.

"When a patient has been subject frequently to periodical attacks of gout, it readily adopts the signs of this indisposition; but the patient complains that his gout is not so perfect as it ought to be, that the disease lingers, that he does not find the relief he has formerly experienced at the same period of a fit of the gout, and grows wearied at length of repeated efforts made, in vain, to assist him in throwing out a more vigorous disease.

"When it combines itself with an accidental disorder, a common cold for instance, the symptoms of catarrh continue to manifest themselves, and even to predominate throughout the greater period of the duration of the climacteric disease; and so hide from the patient and his friends, who wonder he does not get well, a sense of his danger, until at length the extraordinary protraction of the complaint, and an unusual decay of flesh and strength, obtrude the painful truth, that there is some deficiency of vital power in his system."

There is a certain portion of good observation in this history: nevertheless we apprehend that the subject is generally more or less understood, and as generally resolved into what it is brought

to in the concluding sentence,—a partial failure of the vital powers; and therefore, that nothing is added, either to the correct view or the practical treatment of this state of habit, by considering this climacteric disease as something distinct from the casual indispositions, which it modifies or mimics; and that all we know, in reality, is that these common maladies, at a certain period of life, are apt to be more severe, tedious, and fatal, than at an earlier age, and in persons whose constitution has been under better regulation. And this opinion is farther confirmed by the only curative precepts, which Sir H. Halford proposes. "Physicians will not expect me to propose a cure for this malady. In fact, I have nothing to offer with confidence, in that view, beyond *a caution*, that the symptoms of the disease be *not* met by *too active* a treatment." Stomachic purgatives and the Bath waters are suggested; but "for the rest," says our author, "the patient must minister to himself." This, however, we apprehend, is a secret, which he only discloses to his brethren.

An anomalous case, which terminated fatally in two days, and was apparently very obscurely illustrated by the symptoms during life, is described by Dr Turner. Dissection is said to have exhibited a gangrenous condition of the adipose membrane surrounding both kidneys.

Dr Satterley has given an ample detail of the symptoms of hydrophobia, as they appeared in a girl, eight years of age, who was carried off so late as the seventh day. Mercury was principally trusted in the treatment, and was freely administered both internally, and by friction; but it never produced ptyalism. Dissection contributed nothing, as usual, to the elucidation of the seat of the malady.

The remaining four papers were the contributions of practitioners not belonging to the college, and consist of "a singular case of stricture and thickening of the ileum," by Dr Combe; a "case of super-fœtation," stated, from a certificate of the lady's husband, by Dr Maton; a case of successive suppurations in the liver, with discharges by an external opening through the parietes of the abdomen, through which at length a very large gall-stone was passed, related by Mr Blagden, surgeon at Petworth; and a paper of Dr Haygarth's, containing a brief attempt to point out the means of discrimination between chronic rheumatism, and gout, acute rheumatism, scrofula, nodosity, white-swelling, and other painful diseases of the joints and muscles.

Such are the contents of this volume, which are diffused over a large portion of paper, by means of type of unusual size and small pages; for this mode of printing, however, the desire of preserving uniformity with the preceding volumes, may perhaps be deemed a sufficient apology.

PART

PART III.

MEDICAL INTELLIGENCE.

Report of Diseases treated at the PUBLIC DISPENSARY, *(near Carey-Street,) London, from August 31st to November 30th 1813.*

Acute Diseases.

	No. of Cases.
Febris	12
—— infantum rem.	30
Hydrocephalus	4
Ophthalmia	2
Epistaxis	2
Cynanche tonsil.	7
Pleuritis	4
Peripneumonia	11
Pertussis	15
Catarrhus	112
Hepatitis	1
Enteritis	3
Peritonitis	4
Dysenteria	12
Scarlatina	12
Erysipelas	2
Roseola	1
Variola	7
Varicella	2
Herpes	1
Rheumatismus acutus	13

Chronic Diseases.

	No. of Cases.
Vertigo	6
Cephalæa	21
Hemicrania	1
Catarrhus chronicus	61
Hæmoptysis	18
Phthisis	23
Palpitatio	4
Dyspepsia	26
Gastrodynia	19
Pyrosis	4
Hæmatemesis	3
Vomitus	1
Hepatalgia	4
Icterus	4
Enterodynia	13
Constipatio	4
Diarrhœa	20
Tænia	1
Marasmus	10
Hæmorrhois	2
Dysuria	2
Enuresis	2
Nephralgia	7
Hæmaturia	1
Hysteralgia	4
Menorrhagia	5
Amenorrhœa	6
Chlorosis	2
Leucorrhœa	12
Hysteria	6
Epilepsia	6
Hypochondriasis	1
Insania	2
Fatuitas	1

Chronic Diseases continued.

	No. of Cases.		No. of Cases.
Rheum. chron.	39	Psoriasis	5
Lumbago et Sciatica	7	Urticaria	6
Pleurodyne	6	Purpura	1
Asthenia	9	Impetigo	4
Anasarca	9	Porrigo	19
Ascites	2	Ecthyma	1
Hydrothorax	1	Scabies	10
Hydrops. ovarii	1	Aphthæ	1
Struma	3	Eczema mercuriale	2
Lichen	2	Rupia	2
Lepra	1	Acne	1

The uniform, dry, though not hot, weather of the preceding month, continued throughout September. In October it became unsettled, the rains were frequent, and accompanied by cold winds; and November has maintained its character of gloom, having been foggy, dark, damp, and cold, from the beginning: towards the conclusion, the fog was combined with a frost, somewhat more severe than is usual at so early a period of the winter. During the greater part of the three months, the wind from the east and north-east has prevailed, producing a chill even in September, and aggravating the effects of the dampness and cold of the past and present months.

The steady and temperate weather of September appears to have been conducive to good health, and especially to have precluded the occurrence of the bilious affections, which frequently occur in the autumnal season: no case of cholera, or bilious fever, was seen in that month, and the ordinary bowel-complaints were even below the average of these every-day maladies. The wet and unsettled state of October occasioned an increase of disease; but the cold and fogs of November have more manifestly multiplied every form of indisposition. Of the 173 cases of acute and chronic catarrh, included in the preceding list, only 33 cases occurred in September, and upwards of 100 of the remainder, together with all the cases of peripneumony, have taken place within the present month. In many instances, the greater part of a family suffered the catarrhal attacks together, or in quick succession. During the same period, there has been a remarkable tendency in the fevers, which have occurred, to affections of the bowels and peritonæum. In several cases, this amounted to actual peritonitis, and enteritis; but in others, which commenced as simple idiopathic fever, great pain and soreness on pressure ensued in the course of the complaint, requiring leeches or repeated

ed blisters to be applied to the abdomen, while at the same time the contents of the bowels were constantly evacuated by gentle purgatives. The alvine excretions were, in all these cases, of an unnatural consistence, dark, and offensive; and the irritation arising from them appeared to be the cause of the supervening peritoneal soreness, which so often shewed itself. This form of the complaint was particularly prevalent among young people, and was probably but a modification of the infantile remittent, which attacks, and which has during the same time indeed been unusually frequent among, subjects still younger. It appeared, that a constant action upon the bowels, by small doses of calomel with antimony, or with the extract of poppy where the irritation was great, produced a more decided amelioration of the symptoms, than active purgation, employed at intervals. The latter mode of evacuation seemed to harass the patients without correcting the morbid condition of the excretions, and was followed, in some instances, by an increased dryness of the mouth, with a redness and choppy state of the tongue, and increased sharpness of the pulse, which seemed to imply an increase of the irritation of the alimentary canal.

In one case of fever, in which the bowels had been for some time considerably deranged, without any symptom of cerebral excitement, the patient appeared likely to recover, as the derangement of the bowels was subdued, and did not recur. Nevertheless, he remained in a feeble, fretful, and irritable state, becoming at times confused in his intellect, and talking incoherently when alone, though answering inquiries distinctly, his pulse beating about 100 times in a minute, and his distress being confined, as he affirmed, to the aching of his back and limbs. About the end of the fifth week, he became rather tremulous, and in a day or two his pupils were observed to be dilated under the impression of a strong light. His sight became imperfect, at the end of the same week, but without strabismus, and he died. The brain was examined, and a large collection of water was found exterior to that organ, between its investing membranes, which were more vascular than usual; but the ventricles contained very little fluid. Was this vascularity and effusion from the membranes of the cerebrum the consequence of the continued excitement of the fever, induced by abdominal irritation, as in the case of some of the modifications of hydrocephalus in children? The patient was a young man of 18. He never complained of his head; and the occasional delirium, and at one time, about a fortnight before his death, a most profuse sweating from the head and face, were the only indications of irregular action in the cranium.

In

In some of the filthy courts in Saffron-Hill, near Hatton-Garden, which are inhabited almost exclusively by the lowest Irish labourers, typhus has been considerably more frequent during the months of October and November, than for several years past. It has spread rapidly and fatally in several houses, and would probably have committed great ravages among that dirty and crowded population, if the patients had not been received into the House of Recovery in Grays Inn Lane, and the processes of lime-washing, and fumigating the infected houses been diligently employed.

Although several cases of scarlatina have come under our notice, we have not seen any instance of measles. As far as the experience of an institution, of the extent to which this Dispensary goes, can be deemed of weight, it may be mentioned, that the evidence of our records does not tend to prove, with respect to that district of London which it comprehends, that measles are either more prevalent or more fatal than formerly. It is, in fact, a long time since we witnessed the death of a patient from that disease. It would be important to collect the testimonies of practitioners, upon this point, in different parts of the country, in order to ascertain whether the fact, which Dr Watt has proved to have occurred at Glasgow, be really true with respect to other districts, or whether it may not be partial, and have arisen from some local circumstances belonging to that city exclusively. I am in possession of some evidence which tends to prove that the fact is not general. It must be expected, that where the small-pox has been generally superseded by vaccination, and thus many weakly infants have been preserved during the first years of life, yet, on the whole, the number of those who attain the age of puberty may not be much increased; and among the diseases, from which they are liable to suffer, the measles and their sequelæ will probably be as trying to their feeble constitutions, and contribute to carry off as large a share, as any other diseases of childhood.

Bloomsbury Square, Nov. 30, 1813. T. B.

Vaccination.

MR RIGBY of Norwich has printed an interesting Report of the Pauper Vaccination in that city, from August 10th 1812, to August 12th 1813. The plan which he suggested was, that each individual vaccinated should receive half-a-crown from the Court of Guardians of the Poor. The vaccination commenced immediately, and has been continued with the most important

* At present, (December 15,) the measles are epidemic in Edinburgh and its vicinity, and are uncommonly mild.—*Editor.*

portant result to humanity. Two thousand three hundred and ninety-one individuals were vaccinated in the course of one year, between the 10th August 1812, and the 10th of August 1813. Of these, 1508 have each received half-a-crown, offered by the Court of Guardians, amounting to the sum of L.188, 10s.

Since the introduction of vaccination, there has been no instance, in this county at least, where, in a place of the like population, so many poor persons have, within the same period of time, received the benefit of this inestimable discovery; and certainly there has been no instance in England, in which so many have been vaccinated at so small an expence to the public. We wish this important fact may be generally known, that other places may profit by the example of Norwich, and adopt the same means for the diminution of human misery, and for the preservation of human life.

Report.

Having, with every friend to humanity, lamented the difficulty of extending the benefit of vaccination to the lower classes of society, which, for many years, have, in this country, been the principal sufferers from small-pox, and having found the exertions of individuals, and even the establishment of societies, one of which, a few years ago, was formed in this city, unavailing, sufficiently to interest the poor on this important subject, I considered it indispensable, in any attempt to effect an extensive pauper vaccination, to profit of the influence and authority of those bodies of men to whom, in most large cities, the law has peculiarly entrusted the care of the poor; and, at the same time, to endeavour to obtain the consent of the poor to such a measure, by holding out to them the additional inducement of a reward.

In this city the important duty of taking care of the poor rests with 60 citizens, called Guardians of the Poor, and who hold a monthly court; some of these are magistrates, and being myself, as a magistrate, a member of this court, I have several times urged it to take this subject into consideration.

In the beginning of August, 1812, having information that the small-pox had appeared in some neighbouring villages, I was induced to renew my earnest application to the court on the subject, and I succeeded in obtaining not only its sanction for a general gratuitous vaccination of the poor, but its concurrence in my proposal that each individual vaccinated should receive half-a-crown. This resolution of the court was publicly notified, and the three city surgeons were directed to vaccinate all poor persons, resident in the city, who should apply, whether they, parochially, belonged to it or not.

The vaccination commenced immediately, and the readiness with which the poor submitted to it is manifest, from 754 persons, principally children, having been vaccinated from the 10th of August to the 10th of September. It has been continued from that time to the present, with the exception only of the month of January, when all alarm respecting small-pox had subsided; and the result has been most important to humanity, 2391 individuals having, in consequence of this measure, had the benefit of vaccination, between the 10th of August, 1812, and the 10th of August, 1813; which is, probably, a greater number than, within the same period, have been vaccinated in any other place, in this country, of like population.

Of these 1508 have each received half-a-crown, the court having paid the sum of L. 188, 10s. for this purpose.

As before observed, the vaccination was first had recourse to in consequence

of the near approach of the small-pox. The promptness with which it was undertaken, and the considerable numbers which were immediately vaccinated, seem effectually, at that time, to have prevented its spreading in the city, though a case of natural small-pox, traced from a neighbouring village, had occurred, and an additional source of infection had subsequently been introduced by variolous inoculation, for until the month of February not a single case of the disease existed, at which time 1415 had been vaccinated.

In the beginning of February a soldier's wife, who had passed through Longon, with three children, came into the city—her eldest boy was full of the small-pox, and the two other children were sickening with it, all of them having caught it in London. This unfortunate fact was soon made public, and the vaccination immediately again had recourse to; but the small-pox, on this occasion, soon found its way among the unvaccinated, and several children were sacrificed to it within a few weeks; for it had appeared, nearly at the same time, in a different part of the city, and it was ascertained to have been, also, brought thither by another unfortunate communication with the metropolis. The number of persons who had not profited of the vaccination in the preceding autumn, proved now to be greater than was expected; for calculating on the probable average number of annual births in the lower classes, and on the supposed number of individuals who had the small-pox in the years 1808 and 1809, I had estimated the number liable to the disease, in the beginning of August 1812, not to have exceeded 1300.

Though many of the poor were daily vaccinated, more, indeed, than I still calculated to be liable to the small-pox, there was an unfortunate number who yet neglected the boon, and among these the disease has, from that time to the present, spread itself, and I regret to record that, from the 10th of February to the 3d of September, 65 deaths have occurred from it. How much greater the sacrifice of human life would have been, had not so extensive a vaccination taken place, may easily be conjectured—had those, who were vaccinated, been liable to take the infection, the deaths would, probably, have exceeded 400.

During this time, I am sorry to say, there were many instances of the grossest carelessness in the exposure of patients, in all stages of the disease, in the most public streets. It was also admitted into several public-houses, the resort of country persons, and thence communicated to some neighbouring villages. I failed, in an application to the acting magistrate, to take some steps to ascertain what public-houses were thus infected, and to guard strangers, liable to the disease, against entering them, as he conceived there was no law to countenance such an interference.

The vaccination, in the preceding autumn, having been so completely successful in preventing the spread of the small-pox, was, unquestionably, a most gratifying circumstance; but though during the subsequent vaccination, from February to August, the disease still made a fatal progress, the melancholy fact has afforded an irrefragable proof of the protecting power of vaccination: during this period probably not fewer than 400 individuals have had the small-pox; there has likewise been no intermission of the disease—it has been constantly spreading, and on many occasions, patients, as before observed, have been publicly exposed. Of the 2391 vaccinated during the year, it may be assumed, that at least 2000 have been resident in the city since February, and consequently equally exposed to an infectious atmosphere as the unvaccinated, and yet but one single instance, in that number, has occurred, in which the protecting influence of vaccination has been suspected, and this has been clearly ascertained to have been a case of premature vesicle, which suddenly rose, soon disappeared, and evidently produced no constitutional affection *. Whereas every

* This was a child of Mrs Gostling, in Grant's Yard, St John's of Madder-

every unvaccinated person, thus exposed, has probably taken the disease, and I should fear, of those remaining at present uninfected, if not promptly vaccinated, that very few will escape.

From the intercourse between Norwich and the neighbouring villages, the small-pox was soon introduced into them, and has since spread into various parts of the country: it has also prevailed much, and been very fatal at Yarmouth, where few have been vaccinated; many of the local militia were infected there, and communicated it, on their return, to their respective villages. Its progress has also been much accelerated, and its diffusion promoted, by the unjustifiable, and much to be reprobated practice of variolous inoculation. I lament to say, that some professional men have allowed themselves to inoculate for small-pox; but it has been principally done by despicable empirics, itinerants, even shoemakers, and old women; and in some instances, it has been ordered by ignorant overseers of parishes.

In such a disastrous state of the disease, spreading, as it is, at this time, through many populous districts, a general gratuitous vaccination of the poor cannot be too much urged. The signal success of the measure in Norwich has at once established its practicability, and shewn that the means of effecting it are within the reach of every parish in the kingdom. That these means are not of difficult application is evident from its having been there carried through without the smallest agitation of the public mind—without any interruption to public business—with even but little loss of time to the poor themselves—and comparatively with little professional inconvenience. And that it is not objectionable on account of the parochial expence incurred by it, it is equally evident, from 2391 having been vaccinated at an expence to the city of only L.188, 10s. and against this must be placed the expence which would have accrued to the city, had even half that number taken the small-pox. The vaccination of eight in a family, each taking the reward, brings an expence of only L. 1; but should four only in a family sicken with this disease, an expence to the amount of several pounds would, probably, be incurred. The deraugement of a poor family, by the introduction of small-pox, is of the most distressing kind; an immediate suspension of labour takes place; the time and attention of every one are directed to the poor sufferers; increased wants arise; the necessaries for the sick are of a more expensive kind; the means of supplying them cease; the wretched father can earn nothing, he has neither time nor ability to work—he is compelled to apply for parochial relief; and though parishes often give with too sparing a hand, on no occasions have parochial burdens been more severely felt, than when the small-pox has spread through a district.

It has been objected to this measure, that medical men cannot be expected always to vaccinate the poor without some remuneration. In all places of considerable population, the medical care of the poor is contracted for, at a fixed annual stipend; and though I fear, there are few instances of this kind, in which surgeons are paid with adequate liberality, yet when a parish, or a district, is taken,

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market; it had been vaccinated in April; I visited it on the 29th of July; there were many recent vestiges of distinct variolæ upon it. On examining the vaccinated arm, I could detect no cicatrix, and Mrs Gostling, unasked, said she attributed the failure to the vesicle having risen sooner, subsided sooner, and having been less than those in her other three children, who were vaccinated at the same time, and who have all resisted the infection.

As a singular coincidence, I would observe, that about this time my friend Mr Chandler, of St Faith's, called upon me to say that he had just seen a patient under small-pox, who had been innoculated by him, for that disease, nine years

taken, it becomes an object of economy and policy to vaccinate every pauper, rather than risk the expence, trouble, and I may certainly add, the anxiety of attending them under so horrid a disease. On this ground, I am persuaded, there is nothing to fear. There has been, comparatively, little reluctance shewn by medical men, to extend the great benefits of the Jennerian discovery; on the contrary, with few exceptions, much good sense has been evinced in their early appreciation of its high value, and much zeal and personal exertion have been used by them, to establish and extend its beneficial influence in society.

But still more effectually to prevent the future admission of small-pox, and to increase the probability of its ultimate extinction, an event which every friend of humanity will indulge in anticipating, more is unquestionably necessary. It has been ascertained, that small-pox inoculation is the great means by which the disease is kept in existence*, and that London, through its agency, is the great generating focus of variolous infection, whence, as in its late communication to Norwich, it radiates to every part of the empire. While this practice continues, it will be utterly impossible to extinguish small-pox, or to prevent the occasional alarm even of those who have been vaccinated. It is, therefore, indispensable to the interests of humanity, that the practice should cease; but it is pretty evident, while any pecuniary gain attaches to it, individuals will be found to practice it; and while any prejudices remain against vaccination, which it is is their obvious interest to keep up and increase, there will be no difficulty in finding subjects to practice upon.

Under these circumstances there appears no resource but in legislation; and the accumulated mischiefs which have already resulted from the practice, together with its direct tendency to destroy life, by diffusing a pestilential and dangerous disease, will surely justify the passing a law, imposing a severe penalty on any one, directly or indirectly concerned in the act of variolous inoculation.

The greater number of those who are agents in this mischievous practice, are, I am persuaded, incapable of reasoning on the subject, and probably still less capable of any moral discussion; but those few of the profession, who continue it, assume, as a justification, the alleged insecurity of vaccination; and to establish such an opinion, are loud in their abuse of it, industrious in collecting cases of doubt or error in vaccination, some of which must necessarily occur in very great numbers, in magnifying supposed failures, and in laying them most conspicuously before the public in newspapers, &c.

Though most of these cases of imputed failure have been satisfactorily explained†, an unfavourable impression has, certainly, been made on the public mind, the progress of vaccination has been impeded, and what is to be lamented, doubts and alarms have been excited in the minds of those who have been vaccinated; on this account, I have no hesitation in saying, the opposers to vaccination have much to answer for. Admitting their adduced cases of failure to their utmost extent, vaccination would still be a blessing; it would be no reason for abandoning vaccination, but a powerful one for preventing the introduction of small-pox, and for increasing our efforts to exterminate it; and assuming it possible that the failures were even one in twenty, the means of effecting this important purpose would, obviously, be still increased in the proportion of nineteen to one. I will say further, and say it gravely, that, on a fair comparison of the two inoculations, no man of sound intellect, of correct moral feeling, and who does not prefer his individual interest to the general and more important interests of society, will hesitate to prefer vaccination to variolation.

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* Report of the National Vaccine Establishment, 1813.

† See Norfolk Chronicle, September 4, 1813.

lation. When no other means of lessening the fatality of small-pox than inoculation were known, the practice was justifiable; and had the security which individuals obtained from it, been extended to all classes of society, and no injury done to any, it would have been a general blessing. It is well known, however, that its benefits were ever limited to certain orders, and that the poor never having been inoculated to any extent, not only failed to profit of it, but the disease being constantly kept up by a never-ceasing partial inoculation of their richer neighbours, they were, more than ever, exposed to infection, and the average mortality from it, though almost exclusively confined to them, became even greater than before the discovery of inoculation; population proportionately suffering. With vaccination this could not have been—the protection it gives is without alloy; with it, the security of one is not obtained by exposing others to danger: it is not an infectious disease.

Inoculated small-pox is attended with illness in its eruptive stage, sometimes with severe symptoms in every stage, and it has been calculated to have occasioned death in the proportion of one in 300.

A very few years ago, indeed, inoculated small-pox was much more fatal in several parts of Norfolk—at Elmham, Holt, and Aylsham. In Aylsham, considerable numbers were inoculated, and the deaths were nearly one in 20.

The peculiar constitutional change induced by small-pox, by which scrofula has been so often elicited, is not confined to the natural disease; the inoculated small-pox, when severe, has the same prejudicial influence on the constitution, and this has been one of the popular objections to it. I have only one other observation to make—variolous inoculation does not, in every instance, prevent the future admission of the disease;—there are many well attested cases of its recurrence.

Vaccination, on the contrary, through its whole progress, is attended with very little perceptible illness, there is no eruptive fever, there are no frightful convulsions in children, and as it does not in any degree disturb the system, it cannot be charged with exciting any latent morbid affection; it is wholly free from danger; and notwithstanding the magnified reports of the anti-vaccinists, the instances of its failing to secure against small-pox, are not more than those which have followed variolous inoculation.

From a report to the French government on the general state of vaccination, it appears that the proportion of failures in France has been only as 1 to 381,666*.

In all the reflecting and educated classes, there is little doubt of vaccination being generally adopted. But I am advocating the vaccination of the poor. With them, the means of information, on important subjects, are not very attainable; nor can they, for want of intellectual improvement, readily appreciate new discoveries, at all connected with science. On all subjects involving the happiness of the lower classes, much is expected, from the exerted influence of the higher and more educated classes;—and on this, it would seem their obvious duty, to give them information, to remove their doubts and prejudices, and to offer motives for their consent to a measure of such importance, not only to their individual welfare, but to the general interests of society.—I lament to say, little disposition of this kind has shewn itself; and from what I have observed, I fear it will be difficult to excite among the higher classes such an interest on this subject, as to induce, on their part, any extensive exertion in behalf of pauper vaccination.

The efficiency of a gift offered to the poor, has been unequivocally proved in the late extensive pauper vaccination in Norwich, and there is little doubt but

* National Vaccine Establishment for 1813.

but a similar measure would be equally succesful in other places: but the truth must not be concealed, there appears no disposition to follow this example.—Even in the immediate neighbourhood of Norwich it has not been adopted, nor has the experiment been attempted in any part of the county of Norfolk. I fear, therefore, that even this measure, which promises so effectually to establish vaccination among the poor, will not be generally, if at all, had recourse to, unless enjoined by legislative authority—and if considered as a parochial allowance, there would seem no more objection to parliament directing a reward, on the vaccination of the poor, to be paid in every parish in the empire, than there is in its directing a specific allowance to the wives and children of militia-men.

Under a conviction, therefore, that the destructive progress of the small-pox cannot be arrested, unless variolous inoculation be completely put down; and that no measure is so calculated to produce the general vaccination of the poor, (which seems equally necessary to the extinction of small-pox,) as the giving to each person vaccinated a small pecuniary reward; and being not less satisfied that neither of these measures can be carried into any extensive effect without legislative interference, I trust the subject, important as it is in a moral, and in a national view, will, in due time, engage the attention of Parliament.

Norwich, Sept. 6, 1813.

Extract of a Letter from R. Bythell, Esq. Surgeon, Chester.

Relying on your wonted candour for the insertion of any subject connected with surgery, I am induced to offer a few cursory remarks on the use of the elastic gum catheter, for a length of time, immediately after the operation of lithotomy; which, by a fair trial, I am strongly convinced would be productive of very beneficial effects. Though theoretical study, (not experimental knowledge) chiefly lead me to this proposition, still I am further encouraged with the hope of some practitioner of surgery, of greater abilities and opportunities than myself, acquiring a more substantial proof of its efficacy.

Among the many eminent writers on lithotomy, I cannot find such a practice mentioned; and, in patients I have seen undergo extraction, many unpleasant and dangerous symptoms followed, by the length of time the parts unavoidably injured by the operation required to heal, and, in many cases, from the irritation of the urine before it could be evacuated through its natural canal. As a preventive to these evils, I have therefore proposed the use of this catheter.

In the first instance, I beg leave to say, that wearing the elastic gum catheter, assists greatly in conveying the urine direct from the bladder, and allows those parts wounded by the operation, an opportunity of healing much quicker than otherwise they would do; secondly, it prevents the growth of excresences and other obstructions in the urethra, which are often known to occur, and must undoubtedly be attended with disagreeable consequences; thirdly, the great facility of introducing this catheter in most cases, and the trifling inconveniences attending even

a constant use of it, render it again worthy the attention of any unprejudiced surgeon.

Extract of a Letter from Dr M'Mullin, Physician to the Forces, dated Barbadoes, 2d October 1813.

It is a curious fact, but not, I believe, generally known at home, that in places where the worst fevers are observed, such as Point-a-Petre, Guadaloupe, Prince Rupert's, Dominique, and many others, dysentery rarely appears a severe disease; and, *vice versa*, where the latter is found destructive from its severity, the former is seldom noticed in its worst form. This fact explains the surprise expressed by some, that dysentery, a disease so common in marshy situations, never made its appearance during the Walcheren expedition. This disease, although produced by the same causes as the fevers of the country, yet is always most prevalent where, from the immediate contiguity of mountains, sudden vicissitudes of temperature are experienced.

Extract of a Letter from Richard Hooper, Esq. Surgeon to his Majesty's 19th Regiment, dated Matura, Ceylon, 8th September 1812.

No doubt you have heard of the *sudden* and *dreadful* effect of the land-winds, often depriving an unfortunate person, who may happen to sleep whilst they blow, of the use of an arm, leg, or half the body, or perhaps of every limb; and many such cases occur. Whence does this dreadful effect proceed? During the day, or at least soon after sunrise, the air immediately over the earth becomes exceedingly rarified; then commences the sea-breeze. Soon after the sun sets, the earth, becoming comparatively cool, the chilling and destructive land-breeze sets in, and so powerfully does it act, that lamps, tumblers, and decanters, have been cracked by it when on the table, and numerous persons around. What are the effects of the land-breeze in different parts of the globe? Are they similar?

Extract of a Letter from John Sherwen, M. D.

If I have not already trespassed too much on your patience, I wish further to observe, that the cautions which you have given

at p. 367, respecting the indiscriminate use of the stramonium as a popular remedy for asthma, are well-timed and judicious. I wish it had been added, that every good property of the stramonium may be expected from a similar use of the common white poppy heads; the smoke of which, whether swallowed or inhaled, must be equally anodyne, and less deleterious. To obtain the poppy heads in perfection, they ought to be carefully dried while green, before they are arrived at their greatest magnitude. It is probable that the green leaves carefully dried would also be efficacious. From the dried leaves of digitalis similar effects would probably result; and particularly in that species of asthma connected with œdematous ankles, irregular pulse, and other symptoms of hydrothorax.

The letter, p. 419, addressed to Astley Cooper, Esq. on the cure of curved spine, by B. G. Burroughs, Esq. Clifton, conveys, in the form of query, much useful and seasonable information, and will probably have considerable effect in putting a stop to the absurd practice of condemning debilitated children to additional sources of debility. The time is, I trust, fast approaching, when young persons who are threatened with curvature of the spine will have their irons knocked of, and be suffered to run at large in the pure air of the country, where, with the assistance of a little iron taken inwardly, they may have a fair chance to pick up health and strength, beauty and just proportions.

At p. 447, you have given copious and interesting extracts from Mr Brodie's Experiments and Observations on the Action of Poisons on the Animal System. Perhaps on some future occasion it may occur to that ingenious experimentalist, to ascertain how far the essential oil of fine green tea exceeds or falls short of the essential oil of bitter almonds in poisonous property. It may be easily obtained by distillation from the dried herb, as it comes to market in this country. If it should be found that two drops of the essential oil of fine green tea is sufficient to poison a stout man, it would have some weight in convincing a numerous class of incorrigible tea-drinkers, who fancy they cannot exist without it, that they are every day manufacturing for themselves painful spasms and sleepless nights.

On the Qualifications of Surgeon-Apothecaries.

"Quos Jupiter vult perdere, prius dementat."

Mr Editor.—I have felt no small degree of amusement in reading the several advertisements which have, within a short time

time past, appeared in the public papers, addressed to the "Apothecaries and Surgeon-Apothecaries of England and Wales;" and it appears that it was hoped to obtain the unanimous exertion of the whole of that body, to attempt to procure a bill to pass through both houses of parliament, for several distinct objects, all supposed to tend to ameliorate the condition of that branch of the profession. I am happy, however, at having lately seen an advertisement stating that the bill has been withdrawn. I cannot help, however, expressing my opinions on some of the leading topics of the measure proposed. The bill was, if I mistake not, entitled, "a bill to increase the respectability of persons practising as apothecaries," and particularly interested those residing in London or its environs. But, Sir, is the profession arrived to this? Are we come to such a pitch, that an act of parliament is necessary to support the declining respectability of the profession? if so, I fear that not even an act of parliament can stay its fall; for an act of parliament cannot alter the public opinion, and it is by public opinion that professional men are supported. And what, because the members of the profession feel that the reputation of their art is decreasing, (if it be the case,) is it policy? is it wisdom? is it common prudence, to declare it to all the world? The fact is, Sir, that it is themselves that apothecaries have to blame for the loss of respectability. An honourable profession, carried on with honesty and integrity, can never want respect; but when men descend to mean, mercenary, and dishonourable practices, let the profession be ever so honourable, or patronage ever so great, they must, and will inevitably be disgraced. The odious and pernicious system, which is so universal in the metropolis, of sending in as much medicine as could possibly be crammed down the throat of the patient, has, more than any thing, contributed to the disrepute of apothecaries, and has arrived to such a pitch, that people find it now much more economical to employ a physician, and send his prescriptions to the chemist, than to send for the apothecary. The result of which is, that now chemists are accustomed to the compounding of medicines, and are become adepts in pharmacy. But, let a man only exercise liberal sentiments; let him call, and charge for his visits, (to which very few would object,) and let him honestly send in only as much medicine as is necessary; of course varying the FORM according to the circumstances of the patient;—there is no fear but such a person would succeed in his profession, if other necessary qualifications be not wanting. Besides, it must be recollected that public approbation makes a considerable part of

the reward of medical men. They hold a superior rank; they are received into the superior classes of society, and therefore cannot expect a proportion of pecuniary emolument equal to what, independently of the above, might be a just remuneration*. This brings me to the clause respecting charges. It is intended that medical practitioners should have a certain legal claim; of course, then, the rate must be fixed, but how can that be done? Charges ought not be the same to the poor as to the rich; but, according to the plan proposed, there would be a fixed rate for both. As to midwifery fees, too, they should be constantly varied according to the circumstances of the patient.

If the propossed bill pass, we shall see a scale of charges at the end of every Ladies' Pocket-Book after the Waterman's Fares. The conduct of my brethren, on this occasion, has induced me to apply to them the motto at the head of this letter. I trust, however, that they sufficiently see the bad effects of such a measure, to prevent the renewal of an attempt to carry it into effect. I must trespass on you a few moments longer, to say a little respecting apprentices. I must acknowledge, I think the plan intended to have been adopted could not have the effect imagined. The tax of two guineas would not have deterred persons from entering the profession, and would have been only a burden without any good effect. Had a large sum, for instance L. 100, been proposed, it is not improbable that it might have tended to make the profession more select. But, unless there be a law prohibiting any one to practise medicine or surgery but those who are declared qualified, after being examined by a board appointed for that purpose, and in which the examinations shall extend, not only to professional, but also to classical attainments in general, and which examinations shall be somewhat more than formal;—they may tax; they may have an act of parliament;—but they will not raise the profession to that rank in society, which, as guardians of the public health, they ought to enjoy. This calls loudly and imperiously for legislative authority, and, till that be afforded, empiricism will triumph,—science will be depreciated. I am, &c.

May 22, 1813. CHIRURGO-MEDICUS.

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* Vide Smith's Wealth of Nations, Book I. chap. x. pp. 143, 144, 145.

The

The following Professional Works have been recently Published, several of which we shall take an early opportunity of noticing at greater length.

Anatomy, Human, Comparative, and Pathological.

Outlines of the Anatomy of the Human body in its sound and diseased state, By Alexander Monro, M. D. and F. R. S. E. &c. 3 Volumes 8vo. pp. 568. 438, and 585, and one of plates. Edinburgh, 1813.

Anatomy of the Heart, Cranium, and Brain, adapted to the purposes of the Medical and Surgical Practitioner; to which is added, in notes, Observations on the Laws of Life and Sensation. By Alexander Ramsay, M. D. Lecturer on Anatomy and Physiology in Edinburgh. With a Series of Plates of the Heart, Cranium, and Brain, in imitation of Dissections. pp. 66. and xv. plates. 4to. Edinburgh, 1813.

A Short Description of the Human Muscles, arranged as they appear on Dissection; together with their several Uses, and the Synonyma of the best Authors. By John Innes. 7th Edition, with Plates, descriptive of the Bones and Muscles. 12mo. 7s. 6d.

Companion to the London Dissector, or the art and method of making preparations, &c. 2s. 6d.

A Compendious Treatise of Anatomy, adapted to the Arts of Designing, Painting and Sculpture, on Ten folio Copper-plates, &c. By J. Tinney. 5s.

A Compendium of the Anatomy of the Human Body, intended principally for the Use of Students. By Andrew Fyfe. 5th Edition, improved. 3 vols. 8vo. with plates, 1l. 16s. without plates, 1l. 4s.

Outlines of Comparative Anatomy, intended principally for the use of Students. By Andrew Fyfe. pp. 352. 8vo. Edinburgh, 1813.

As this, like most other elementary works, does not admit of a regular analysis, or detailed criticism, we shall express our opinion of it in a few words, by merely stating, that these Outlines are compiled and composed with the same ability and intimate acquaintance of his subject, which have so long rendered Mr Fyfe's Compendium of Anatomy so deservedly popular; and by transcribing the whole of his preliminary advertisement, as a laudable example of that unassuming modesty, which never fails to secure to its possessor universal respect and esteem. "The author thinks it necessary to acknowledge here, that much of the information contained in the following outlines, is derived from modern zootomists, particularly Blumenbach and Cuvier, whose works deserve to be carefully perused by every one who proposes to make comparative anatomy a particular object of pursuit."

Series of Engravings, by Skelton, accompanied with Explanations, which are intended to illustrate the Morbid Anatomy of some of the most important Parts of the Human Body. By Dr Mathew Baillie. 2d Edit. 4to. 8l. 8s.

Engravings from Specimens of Morbid parts preserved in the author's collection, and selected from the divisions inscribed Urethra, Vesica, Ren, Morbosa et Laesa, containing specimens of every disease which is attended with a change of structure in these parts, and exhibiting the injuries from the Bougie, Catheter, Caustic, Trocar, and Lithotomy Knife incautiously used; with Observations. By Charles Bell. Imperial folio. London, 1813. 1l. 16s.

Physiology and Pathology.

An Inquiry into the Laws of Animal Life, being an Analysis of the Principles of Medical Science, with a view to obtain more satisfactory explanations of the phenomena that present themselves in health and disease. By J. R. Park, M. D. of Jesus College, Cambridge. 8vo. 1l. 5s.

The

The Philosophy of Medicine. By Robert John Thornton. 2 Volumes 8vo. 1l. 12s.

Conspectus Medicinæ Theoreticæ, ad Usum Academicum. Auctore Jacobo Gregory, M. D. Editio 4to. prioribus auctior et emendatior. 8vo. 16s.

A View of the Progress and Present State of Animal Chemistry. By Jons Jacob Berzelius, M. D. translated from the Swedish by Gustavus Brunnmark, D. D. pp. 115. 8vo. London, 1813.

The Principles of Physiological and Physical Science, comprehending the ends for which animated beings were created, and an Examination of the Unnatural and Artificial Systems of Philosophy which now prevail. By Richard Saumarez, Esq. pp. 424. 8vo. London, 1812. 10s. 6d.

To J. C. Lettsom, M. D. F. R. S. F. A. S. &c. &c. &c. President to the Council and Fellows of the Medical Society of London, this Annual Oration, published at their request, is dedicated by their faithful servant, Richard Saumarez. pp. xx and 93. 8vo. London, 1813.

We do not profess to understand Mr Saumarez's philosophy or physiology. This, however, may be owing either to our dulness, or to his having lived a century too soon.

An Essay on the Absorbents, comprising some Observations upon the relative Pathologies and Functions of the Absorbent and Secreting Systems. By Daniel Pring, Member of the Royal College of Surgeons, London, and Surgeon at Bath. pp. 92. 8vo. Bath, 1813. 5s.

A Treatise on the Diseases and Organic Lesions of the Heart and Great Vessels. By J. N. Corvisart, M. D. First Physician to their Imperial and Royal Majesties, &c. Translated from the French by C. H. Hebb, Member of the Royal College of Surgeons in London. pp. 404. 8vo. London, 1813. 10s. 6d.

A very good translation of the valuable work of Corvisart, of which we have already given an analysis. See Edinb. Med. and Surg. Journal, Vol. VII. p. 68.

A Treatise on the Diseases of Arteries and Veins, comprising the Pathology and Treatment of Aneurisms and Wounded Arteries. By Joseph Hodgson, In one Volume, 8vo. Illustrated by a series of Engravings in 4to. *nearly ready.*

Treatise on the Blood, Inflammation, and Gun-shot Wounds. By John Hunter. In 2 vols 8vo. without the Life, 1l. 5s.

Lectures on Inflammation, exhibiting a View of the General Doctrines, Pathological and Practical, of Medical Surgery. By John Thomson, M. D. F. R. S. E. Professor of Surgery to the Royal College of Surgeons, and Regius Professor of Military Surgery in the University of Edinburgh, pp. 649. 8vo. Edinburgh, 1813.

Practice of Medicine.

Culleni Synopsis Noseologiæ Methodicæ Exhibens Clariss. Virorum Sauvagesii, Linnæi, Vogelli, Sagari, et Macbridii, Systemata Nosologica, edidit suumque proprium Systema Nosologicum adjecit. Nova Editio. 8vo. 8s.

The Modern Practice of Physic, exhibiting the characters, causes, symptoms, prognostic, morbid appearances and improved method of treating the diseases of all climates. By Robert Thomas, M. D. 4th Edition, corrected and considerably enlarged. 8vo. 16s.

We are happy to have this opportunity of bestowing our commendation upon a work, which, from its comprehensive nature, does not admit of a regular analysis,

analysis, and of recommending it as probably the best and most complete Vademecum of the Practice of Physic in our language. The improvements in each successive edition, are proofs of the attention and industry of its author.

The Influence of Tropical Climates, more especially the Climate of India, on European Constitutions; the Principal Effects and Diseases thereby induced; their Prevention or Removal; and the Means of preserving Health in Hot Climates; rendered obvious to Europeans of every Capacity. An Essay. By James Johnson, Esq. Surgeon in the Royal Navy. 8vo. London, 1813. 14s. pp. 534.

An Account of the Plague which raged at Moscow 1771, translated from the French, with notes. 8vo. 2s. 6d.

A Brief Description of the Plague; with Observations on its Prevention and Cure. By Richard Pearson, M. D. Member of the Royal College of Physicians. pp. 63. 8vo. London, 1813.

In this short pamphlet, its respectable author has given a summary statement of what is most interesting in regard to the history, propagation, prevention, and cure of the plague; and he conceives, that, under the section which relates to the prevention and cure, he has introduced some new suggestions which may not be deemed altogether unworthy of notice. On the treatment proposed, we shall not venture to give an opinion, but the suggestion of reducing the temperature of the atmosphere, by *drenching* the walls of the houses and the streets repeatedly in the course of the day with water, appears plausible.

Ferriar's Medical Histories and Reflections, Vol. IV. 8vo. 5s.

Treatise on the History, Nature, and Treatment of Chincough, including a variety of cases and dissections; to which is subjoined, an Inquiry into the Relative Mortality of Children, and the numbers who have died under ten years of age in Glasgow during the last ten years. By Robert Watt, M. D. pp. 392. 8vo. Glasgow, 1813. 10s. 6d.

Observations on the Distinguishing Symptoms of three different species of Pulmonary Consumption, the Catarrhal, the Apostematous, and the Tuberculous; with some remarks on the Remedies and Regimen best fitted for the prevention, removal, or alleviation of each species. By Andrew Duncan, senior, M. D. and P. pp. 169. 8vo. Edinburgh, 1813. 6s.

Cursory Remarks on Corpulence, by a Member of the Royal College of Surgeons. 8vo. 2d Edit. 3s.

A Practical Treatise on Hæmoptysis, or Spitting of Blood, shewing the Safety and Efficacy of Emetics, and the pernicious Effect of Blood-letting. By Dr George Rees. 8vo. 2s. 6d.

History of the Walcheren Remittent, commencing with its advanced State, when most dangerous and destructive to the Soldiery, and concluding with its very favourable Termination. By Dr Thomas Wright. 8vo. 10s. 6d.

A Comprehensive View of the Small-pox, Cow-pox, and Chicken-pox; with a concise History of their different Stages and Terminations: proving that the real Small-pox never have occurred more than once in the same Person, nor ever after the Cow-pox. By Dr James Sanders. 8vo. 6s.

Cases of Apoplexy and Lethargy, with Observations upon the Comatose Diseases. By Dr John Cheyne. 8vo. plates. 8s.

A Familiar Treatise on Diseases of Skin, exhibiting a popular View of their respective Symptoms, detailing the Limits of secure Self-treatment, and illustrating the perilous Abuse of indiscriminate Remedies; also Remarks on Bathing, &c. By J. Wilson. 8vo. 5s.

The Medical Guide, for the Use of the Clergy, Heads of Families and Practitioners in Medicine and Surgery. By Dr Richard Reece. 10th Edition, 8vo. 10s. 6d.

A Practical Dictionary of Popular Medicine; comprehending the different Branches of the Healing Art, &c. By Dr Richard Reece. 2d Edit. 8vo. 16s.

Surgery.

First Lines of the Practice of Surgery, designed as an Introduction for Students, and a concise Book of Reference for Practitioners. By Samuel Cooper. 8vo. 3d Edit. improved and enlarged. 15s.

A Dictionary of Practical Surgery; exhibiting the present State of the Principles and Practice of Surgery, collected from the most authentic and original Sources of Information; comprehending also an Account of the Instruments, Remedies, and Applications employed in Surgery, and the Etymology and Signification of the principal Terms, &c. &c. By Samuel Cooper. 2d Edit. 8vo. 1l. 1s.

An Epitome of Modern Surgery. By Samuel Cooper. 8vo. 9s. 6d.

Surgical Cases, with Practical Observations: Part I. By George Fielding, Member of the Royal College of Surgeons, Edinburgh, &c. 8vo. Hull, 1813. 4s.

Dufour on Diseases of the Urethra, particularly describing the various Symptoms attending Strictures, Gleets, Seminal Weakness, &c. &c. sewed 3s.

An Essay on the Shoulder-Joint Operation, principally deduced from Anatomical Observation. By W. W. Fraser. 8vo. London, 1813. 2s.

Thomas Kirkland. An Appendix to an Inquiry into the present State of Medical Surgery; in which the Removal of Obstructions and Inflammation in particular Instances, with the Causes, Nature, Distinctions, and Cure, of Ulcers is considered; taken from his Manuscript, with a Preface, &c. By James Kirkland, Surgeon, &c. 8vo. 6s.

Observations on Ophthalmia and its consequences. By Charles Farrel, M. D. Surgeon to his Majesty's Forces. pp. 138. 8vo. London, 1811. 5s.

Practical Observations on Various Novel Modes of Operating on Cataract, and of forming an Artificial Pupil. By Robert Muter, Holbeach, Member of the College of Surgeons London. pp. 115. 8vo. Wisbech, 1811.

Practical Observations on Ectropium or Eversion of the Eye-lids; with the description of a new operation for the cure of that disease, on the modes of forming an Artificial Pupil, and on Cataract, illustrated with coloured plates. By William Adams, Member R. C. S. London, &c. pp. 252. 8vo. London, 1812. 12s.

A Practical Treatise on Cataract. By John Stevenson, Oculist and Aurist to the Princess of Wales. pp. 123. 8vo. London, 1813.

Midwifery.

Hints for the Treatment of the principal Diseases of Infancy and Childhood, adapted to the Use of Parents. By Dr James Hamilton. 2d Edition. 8vo. 7s.

A Treatise on the Management of Female Complaints, and of Children in early Infancy, &c. By Dr James Hamilton, sen. 7th Edition. 8vo. 10s. 6d.

A Synopsis of the various Kinds of difficult Parturition. By Dr Samuel Merriman. 12mo. 1s. 6d.

A Practical Essay on the more important Complaints peculiar to the Female. By Dr John Robertson. 18mo. 4s.

Materia Medica and Pharmacy.

A System of Materia Medica and Pharmacy, including translations of the Edinburgh, London, and Dublin Pharmacopœias. In 2 volumes. By John Murray, F. R. S. E. &c. pp. 512, 496. 8vo. Edinburgh, 1813. 1l. 1s.

The Edinburgh New Dispensatory, containing the Elements of Materia Medica and Pharmacy, with accurate Translations of the London, Dublin, and Edinburgh Pharmacopœias. By Dr John Thomson, 8vo. 12s.

The Edinburgh New Dispensatory: containing, 1. The Elements of Pharmaceutical Chemistry. 2. The Materia Medica, or the Natural, Pharmaceutical,

cal, and Medical History, of the Substances employed in Medicine. 3. The Pharmaceutical Preparations and Compositions. Including Translations of the latest London, Edinburgh, and Dublin Pharmacopœias. With Tables, and Plates of Chemical Characters and Pharmaceutical Apparatus. By A. Duncan, Jun. M. D. and P. 7th Edit. corrected and enlarged. pp. cxcii and 713 8vo. Edinburgh, 1813. 15s.

Practical Observations on the use and abuse of Cold and Warm Sea-bathing in various diseases, particularly in Scrofulous and Gouty cases. By John Gibney, M. D. Resident Physician at Brighton. pp. 144. 8vo. London, 1813. Price 5s.

Pharmacologia, or the History of Medicinal Substances, in order to enable the Practitioner to prescribe them with Efficacy and Elegance, and to dispense them with Accuracy. By J. A. Paris. 12mo. 6s.

An English Catalogue of Drugs, with their Properties, Doses to Children and Adults, the proper Vehicle for their Exhibition, and retail Price annexed to each Article. By Reece, Burges and Co. 3d Edit. 8vo. 2s. 6d.

The English Olive Tree, or a Treatise on the Use of Oil and the air Bath, and Miscellaneous Remarks on the Prevention and Cure of various Diseases, Gout, Rheumatism, Diabetes, &c. By the Rev. W. M. Trinder. 8vo. 3s.

Medical Police and Jurisprudence.

An Essay on Diet and Regimen, as indispensable to the Recovery and Enjoyment of firm Health, especially to the Indolent, Studious, Delicate, and Invalid, with Cases, &c. By Dr J. M. Adair. 2d Edit. 8vo. 5s.

Medical Admonitions to Families, respecting the Preservation of Health, and the Treatment of the Sick. By James Parkinson. 5th Edit. 8vo. 10s. 6d.

An Examination of the Imposture of Anne Moore, called the Fasting-woman of Tutbury. By Dr Henderson. 8vo. 2s. 6d.

A Statement of Facts relative to the supposed Abstinence of Ann Moore of Tutbury. By the Reverend Ligh Richmond. 8vo. 3s. 6d.

The Accidents of Human Life, with hints for their prevention, or the removal of their consequences. By Newton Bosworth. 18mo. London, 1813. 4s. 6d.

Particulars of a Case of Hydrophobia. By Ryce Wynne. 8vo. 1s. 6d.

An Essay on the Signs of Murder in New Born Children. Translated from the French of D. P. A. O. Mahon, Professor of Forensic Medicine in the Medical School at Paris. By Christopher Johnson, Surgeon, Lancaster, &c. with a Preface and notes by the Translator. pp. 114. 8vo. Lancaster, 1813.

Tyrocinium Medicum, or a dissertation on the Duties of a Youth Apprenticed to the Medical Profession. By William Chamberlaine, Member of the Royal College of Surgeons, &c. pp. 253. 12mo. London, 1812.

A serious parody upon Lord Chesterfield, which has amused us as often as we have looked into it, and will be perused by those for whom it is intended with benefit to themselves. Mr Chamberlaine gives full and particular directions how an apothecary's apprentice may become a polite gentleman, and a profitable servant, and ultimately an adept in squeezing as much money out of his patients, as they will part with. It, however, contains much useful information, to be found nowhere else.

Aesculapian Secrets Revealed, or Friendly Hints and Admonitions, addressed to Gentlemen of the Medical Profession, and the Public in general, containing Maxims of indispensable consequence, which, if attended to, will effectually conduct the Practitioner, by the most simple and unerring Method, to the highest pinnacle of fame, honour, and independence. By Peter MacFlogg'em, Esq. M. D. F. R. S. LL. D. and A. S. S. Fellow and Honorary Associate of all the Medical Societies in London. pp. 226, 12mo. London, 1813. 6s.

There

There may be wit in this volume, but we have not discovered it. Altogether, it appears to us to be the most absurd and contemptible rhapsody that ever fell into our hands.

A Letter on the State and Condition of Apothecaries, with proposals for making their offices more respectable and more beneficial to the public. Addressed to Pharmacepola Verus, by a true Surgeon. 1s. 6d.

Veterinary Medicine.

An Inquiry into the Structure and Animal Economy of the Horse, comprehending the Diseases to which his Limbs and Feet are subject, with Directions for Shoeing, and pointing out his Age until his Twelfth Year, &c. By Richard Lawrence. 4to. 1l. 1s.

A Treatise on Glanders, being chiefly a plain narrative of facts, elucidating the cause, prevention, and cure of that destructive malady. By Thomas Smith, late Veterinary Surgeon to 2d Regiment Dragoon Guards. pp. 154. 8vo. London, 1813. Price 7s. 6d.

We have hitherto paid too little attention to this department of our Journal, but we trust that, in future, we shall be able to render it tolerably complete; and, with this view, we request that Authors and Publishers will send, (post paid) to the care of Messrs CONSTABLE & Co. Edinburgh; LONGMAN HURST, REES, ORME, & BROWN, London; or GILBERT & HODGES, Dublin, accurate transcripts of the title-pages, with the prices and number of pages, of new works, and improved editions of works in any way connected with the profession.

Mr Wardrop has in the press a second and third volume of his "Essays on the Morbid Anatomy of the Human Eye." The second volume contains an account of all those diseases of the Eye and its appendages which were not given in the first volume, illustrated by numerous coloured engravings by eminent artists. The third volume contains an account of the *treatment* of the diseases of the eye.

Mr Thomas Baynton has a work in the press that will be speedily published, on a new and successful method of treating diseases of the spine.

Mr Wishart's Translation of Scarpa on Hernia, is almost ready for publication.

Several notices of the opening of medical classes in London arrived after the number for October was printed off. To prevent disappointment, they should be sent at least six weeks before the date of publication.

Communications have been received from Dr AYRE of Hull; Dr FULLER of Sunderland, &c.

*** Communications may be addressed to the Editors, to the care of Messrs CONSTABLE & Co. Edinburgh; Messrs LONGMAN, HURST, REES, ORME, & BROWN, London; and GILBERT & HODGES, Dublin.

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THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. APRIL 1814.

PART I.

ORIGINAL COMMUNICATIONS.

I.

Observations on the Plague as it lately occurred in Malta. By A. Brooke Faulkner, Physician to the Forces.

Upon the pathology and treatment of this most unmanageable of all diseases incident to humanity—the plague—the late experience of medical men in this island, I regret to find, contributes but little to what is already so imperfectly known on the subject. Every attempt to accommodate its phenomena to the operation of general laws, or to discover any thing approaching to a successful method of cure, either by experiment or speculation, has shared the same unfortunate fate as in all former ages.

Such detached notes as I have been enabled to collect from personal observation and practice, I shall here throw together, rather to satisfy the curiosity of my friends who have solicited the offering, than with any expectation of materially benefiting the world by their importance.

Under the disadvantage of hindrance from visiting patients in the Maltese pest-hospitals, I am necessarily precluded from communicating many facts from my own experience, which I should otherwise have been enabled to do. This privation, I have, however, attempted to make up for by conferences with some of the most intelligent practitioners in the island; the results of

which, together with my own observations and reflections, it will here be my purpose to render some account of.

The plague, above every other distemper with which I am acquainted, either by reading or experience, is one of the most irregular type, modified in its symptoms and appearances to a degree surpassing all belief and every attempt to explain, apparently by difference of constitution, age, temperament, manner of life, and other constitutional peculiarities in its victims. Most usually, however, its first approaches are marked by some of the following signs: headach, sickness, debility, stupor, rigors, vertigo, vomiting (of a vitiated bilious matter); pain of the back opposite to the region of the kidneys; suffusion of the eyes; an appearance of countenance resembling that of a person recovering out of a severe fit of intoxication, and inability to stand upright, not unlike what occurs in that state; quick pulse; whiteness of tongue; costiveness; occasionally diarrhœa. Fewer or more of these symptoms, for the most part, characterize the disorder very early after its accession. But it sometimes happens, that, without any other previous indication, glandular tumours give the first alarm of its presence.

Of the state of the pulse, I regret to say, I am not prepared to give any satisfactory account, as medical men were all alike absolutely interdicted from informing themselves upon the state of this function. By intelligent authorities, I have been confidently informed, that, at an advanced period of the disease, the pulse was so very much accelerated, as to render every attempt to count it almost impossible, the pulsations feeling rather to succeed each other in a continued stream than marked by any distinct intervals.

Apparently modified by the same peculiarities of constitution, &c. the character of the concomitant fever becomes extremely irregular, assuming every shade of variety from synocha down to the lowest degree of typhus, and, in some instances, having accessions of rigor, not unlike an irregular species of intermittent.

Thirst, the never-failing attendant on all other diseases which are accompanied with febrile symptoms, is not invariably present in the plague, even in the most urgent cases. In patients under excruciating distress, and at the very acmè of the disorder, I have known this symptom either wholly wanting or very moderate. The like remark holds of want of appetite. Throughout the disease, this function is not only not impaired, but augmented to a degree bordering on voracity.

The alvine evacuations are commonly of a darker appearance than natural. I have observed them of a greenish tinge, and mixed

mixed with scybala. When lumbrici accompany this state of the bowels, which is not unfrequently the case, they indicate a very unfavourable disease. This greenish colour of the stools was particularly remarkable in the patients in whom I observed voracity of appetite, and would seem to shew that this symptom was occasioned by the generation of a strong acid in the stomach and primæ viæ.

It is a striking circumstance, that patients, often at the commencement of the disease, are averse to admit of their being ill, and that they persist tenaciously in holding the same opinion, until matters proceed so far as to render any confession on the subject unnecessary.

The suddenness with which the plague attacks its victims is altogether incredible, persons being known to enjoy every appearance of good health a few minutes before its attack. The fatal termination is often not less rapid, occurring in a few hours. Certain cases, (though comparatively few,) have been protracted for a fortnight or three weeks before the patients have enjoyed any perfect exemption from danger; and, in some rare instances, after every apparent danger was survived, a phthisis or dropsy has supervened and proved fatal. This occurrence takes place, as might be expected, in persons of a very lax fibre and debilitated habit. Yet, seldom do seven days elapse, upon an average, from the first period of confinement, until the prognosis is decided with sufficient certainty.

Death very rarely follows a gradual extinction of the powers of life. In the greater number of cases it is ushered in unexpectedly by some violent delirious effort, or suddenly terminated in convulsions.

Delirium attends the plague in all its varieties and gradations, though in some cases there is no observable disturbance of the mental faculties at any time of the disease. In others this symptom is diversified from a state of the lowest insensibility, to the very highest imaginable degree of excitement, resembling the fury, and accompanied with the actions of the maniac.

The state of the urine is various, being sometimes crude, at others high-coloured, and differing alike in point of quantity; but I did not see, nor could I learn, that the remission or aggravation of symptoms was indicated by any visible alteration in the appearance of this evacuation.

The accounts in circulation relative to the interval that passes from the first application of the pestilential poison until the production of the disease, are very discrepant. Whilst some persons are stated to have been attacked almost immediately after the noxious contact, others were represented to have continued

well

well an incredible length of time, before any symptoms became evident. But I am certain, that no conclusive experiments have been instituted into this matter, as the people always strove to conceal their illness, along with the history of it, as long as in their power, knowing perhaps what they had to expect from the disclosure, and the consequences of removal to the pest-hospital,—that bourne from whence so few travellers returned.

About sixteen or twenty days are generally considered to be the greatest interval between the application of the cause and the first evidence of the complaint.

The matter of infection, of whatever nature it is, seems certainly to be of a very specific kind, as only communicable by the application of infected substances to the skin: at least the present times have as yet afforded no good reason for confidently believing, that it is received through any other medium than that of contact with the human body, *directly* or *intermediately*.

The many instances which have happened of whole families escaping the infection, after one or two of them had been removed labouring under severe symptoms of the disease, would seem to prove beyond a doubt, that a certain concurring state of the body was absolutely necessary for the reception of the pestilential virus. Several very remarkable cases of this were narrated to me, and some I have myself noted, of patients being taken from the bosom of their families in the most distressing disease, and with perfect impunity to those with whom they had communicated;—children from their mothers, and husbands from their wives. Yet these families had used no kind of precaution whatever, not so much as an attention to common cleanliness. To enter into a detail of such instances, would itself be a voluminous labour; they are matter of the most public notoriety. To go no farther than the regiment of De Rolls, the medical care of which was in a great measure entrusted to myself, I have known cases of plague taken out of the very heart of a company, and of so urgent a kind, as to prove fatal in a day or two, whilst the rest of their comrades continued to enjoy very perfect health. Neither was there here any obvious cause to which their escape could be ascribed,—oil frictions, fumigations, or any other description of precaution, prevention, or antidote. My own Calesse man and two of his children died of the disease, but his wife perfectly escaped, as did also his brother-in-law, who was constantly in the habit of familiar intimacy with them all. I am not acquainted with a poor family in Malta, who are more negligent of personal cleanliness than this I am speaking of. As to any precautions of rubbing with oil or vinegar, they were wholly

wholly ignorant of any thing of the kind, until I directed them, considerably subsequent to their misfortune.

Though for my own part I have a strong persuasion that the infection of plague may be not less liable to be received by that species of contact which can reasonably enough be imagined to take place by inhalation, I am not enabled to adduce evidence sufficiently satisfactory to decide the fact. I find there still exists a great contrariety of sentiment upon the subject. To determine the truth with precision would require a deliberate experiment, which I fear few would be disposed to make. The escapes of the attendants, who were necessarily much and closely engaged about the persons of the infected, holds out presumptive proof, that the matter of infection is at least not of so virulent a kind as to be carried delcteriously any great distance in the atmosphere.

Instances of reinfection have not been frequent, I am informed; yet, in one individual, I knew it to take place three times, in each succeeding one the symptoms growing milder. The example here alluded to furnishes an exception to the general remark, that the plague proves most fatal to those of a lax fibre. This person was of a remarkably delicate habit. Upon the comparative degrees of susceptibility of men, women, and children, I could not obtain any precise intelligence, or rather the accounts given to me were so much at variance, that little could be inferred from them. I think the balance was, upon the whole, rather in favour of the opinion recorded by some early writers, viz. that the two latter are the most liable to be infected, and that the debilitated of the same sex are more susceptible than the most robust.

As to the diagnosis in plague, it is often a point of much embarrassment to pronounce with absolute certainty upon. The muddy dull eye, described by Russell, is, in my opinion, without doubt, one of the most leading and faithful monitors of its presence. I seldom found myself mistaken, in regarding a case as plague, when there was any unusual whiteness of tongue, accompanied with this appearance of the eye, even though there was no intumescence or redness about the glands visible, and the patient did not confess any complaint. When this appearance of the eye and tongue concurred with glandular swellings or external tumours, the point was placed beyond a doubt.

With regard to the prognosis, I think it may be generally assumed, that the less severe the affection of the brain is, and the more remote the symptoms from indicating a state of putridity, favourable hopes may be proportionably entertained for the issue. But here, too, our expectations were often suddenly frustrated;

some cases which have gone on to prosper for several days terminating unexpectedly in death, whilst others, which continued for whole weeks, apparently in the most hopeless state, have as agreeably disappointed our fears, and been ultimately restored to pristine health and vigour.

Buboes, when they come out tardily, denote commonly an ungracious disease. When they go on speedily to suppuration, or when they recede whilst the other symptoms meliorate, there is the fairest hope of a prosperous event. The same may be stated of the condition of the carbuncles.

I need hardly observe that subsultus tendinum is a symptom which augurs an irrevocable fate.

It has been communicated to me with more confidence than most other peculiarities of the plague, that the nearer the glandular or other tumours or external appearances approach to the head, the prognosis is proportionally unpromising.

A diarrhœa coming on early to any great extent, when the brain continues much affected, is accounted among the signa infaustissima.

The spontaneous supervention of an early perspiration is a flattering omen. It seldom fails to relieve the febrile symptoms, and to be followed by general amendment.

In some cases the glandular swellings do not make their appearance until very near to death, when some slight tumefaction is to be seen by a close inspection. Instances are not wanting after death, of these swellings subsiding entirely out of sight. After death, several cases have occurred of petechiæ manifesting themselves for the first time, when I have seen, also, very broad livid spots, covering the whole of the lower part of the trunk and extremities, of about the diameter of a quarter of an inch each.

The petechiæ which I observed upon the dead were various in point of size and colour; on one of a dark or dusky brown; on another, inclining more or less to lividity; on one they were almost imperceptibly minute; on another as large as flea-bites. Their seat is commonly over the breast, arms, or wrists, but they come out likewise upon the back or lower extremities.

The carbuncles that fell under my observation were of that kind described by authors as the wet carbuncle, sloughing into very deep sores, and attended, during the progress of inflammation, with an extremely painful burning sensation. At first they arise like a phlegmon, gradually acquiring a diffused and highly inflamed base, and having, not far from the apex, a concentric areola of a deep livid, and more internally of a cineritious colour, and a glossy appearance. The eruption of carbuncles is not confined to any particular part of the body or limbs, though more

more commonly their situation is upon some part of the extremities. Of the *dry* carbuncle, or that occasioned by the confluence of the pestilential eruptions, called blains, I have not had personal experience, nor have they been very often noticed by others. I have, however, received some account of them, as they occurred in a few cases, and which pretty exactly corresponds with the description given of them by authors, being of a dark gangrenous colour, without much pain, with little or no inflammation, and not elevated above the surface of the skin. They are accounted to designate a very unfavourable disease.

Irregular and inconstant as are all the phenomena of this perplexing disease, its varieties, as remarked on the late occasion, may, I think, be classified under the three following species, viz.

I. That in which, at the first attack, the energy of the brain and nervous system is greatly impaired, indicated by coma, slow, drawling, or interrupted utterance. In this description of the disease, the tongue is white, but little loaded with sordes, and usually clean, more or less, towards the centre and extremity; the anxiety is great; cast of countenance pale; stomach extremely irritable, and the strength much impaired. Rigors and pain in the lower part of the back are among the early precursors of the other symptoms. This was observed to be the most fatal species of plague, and prevailed chiefly at the commencement of the late disasters. Those who were thus affected sometimes died in the course of a few hours, and with petechiæ.

II. The next species I would describe is, that in which the state of the brain is the very reverse of what takes place in the former, the symptoms generally denoting a high degree of excitement: the pain of the head is intense; thirst frequently considerable, though sometimes wanting; countenance flushed, and utterance hurried. The attack is ushered in by the same rigours and pain of back as in the foregoing. Epistaxis not unfrequently occurs in this class of the disorder. The glandular swellings come out very tardily, and, after appearing, recede again without any remission of the general symptoms. Carbuncles arise over different parts of the body or extremities, which are rapidly disposed to gangrenous inflammation. The delirium continues extremely high and uninterrupted, and the patient perishes in the course of two or three days. Sometimes he lingers so far as the seventh, yet rarely beyond this period, without some signs of amendment. Of this second description, the examples have been very numerous, and were nearly as fatal as the preceding. In the countenances of some, just previous to the accession of the more violent symptoms, there is an appearance of despair and horror

horror which baffles all description, and can never well be mistaken by those who have seen it once.

III. The third species which I would enumerate, is nearly akin to the last, only the symptoms are much milder, and the brain comparatively little affected. The buboes and other tumours go on more readily and kindly to suppuration, and by a prompt and early employment of remedies, to assist the salutary operation of nature, the patient has a tolerable chance of surviving. Cases of this class are often so mild, that persons have been known to walk about in seeming good health, and without any evident inconvenience from the buboes. Of this last species, the instances have, thank God, not been unfrequent, chiefly occurring towards the declension of the malady.

With relation to the means of prevention or cure, I have, alas, little to offer which can flatter our expectations of subduing the enemy, or disarming him of his terrors.

Of the preservatives which have been most universally in repute, washing with soap and water, rubbing with vinegar and with oil, are the principal. But I am yet to learn of any well authenticated instances, which point out that any means of prevention have succeeded so well as a strict and unrelaxing attention to cleanliness, and shunning of contact with persons or things imbued with the contagion. These are the only means I have ever used myself, if I only except the oiled silk dress which I took occasion to recommend for the attendants of every description, in our military plague-hospital. Even without the dress, however, I have been in the habit of approaching close to patients in the most advanced stages of the disease, with no other safeguard than that of a sponge dipped in vinegar held to my face. In the oiled silk dresses, as affording a protection against contact of infected substances, I have the greatest confidence. When persons are constantly obliged to be about the sick, and who have not a disposition to strict personal cleanliness, these dresses are, in my opinion, altogether invaluable as an armour.

Upon the preservative virtues supposed to be possessed by oil frictions I have many doubts, notwithstanding the terms in which they are mentioned by so respectable an authority as Mr Baldwin, and their antiquity, being recommended by Celsus. I have made much inquiry into the subject, without being able, in any degree, to convince myself of the justice of the opinion which has been entertained of its anti-pestilential efficacy. So early as the time of Diemerbroek, that celebrated professor advised stimulant oil frictions, both as a preventive and remedy in the plague, for which purpose he particularly recommended a combination of the

the oils of nutmeg, amber, cinnamon, mace, cloves, and some others, with which those who were obliged to go out of doors, or to be otherwise exposed to infection, were directed to rub every exposed part of the body, and over the region of the stomach. In perusing a late authority on this subject, I find there was an oil called, κατ' ἐξοχήν, *oleo di Gran Duca*, much in repute in Italy as a preventive. I have not been able to ascertain its composition, or indeed whether it was simple or compound. Thus far authorities are in favour of the practice. But, when it is considered what a great length of time has elapsed, since even the latest of the above-cited authors flourished, and what havoc the plague has gone on to spread through the world since, I fear mankind have not much reason to confide greatly in the conservative virtues of oil, or any other antidote with which books make us acquainted. Like the amulet's charm and St John's books, which have in their day, too, imposed on the credulity of man, it is to be feared that the belief in the efficacy of oil is revived only in its turn to fall into equal oblivion.

Orræus, physician to the late Empress Catherine of Russia, pointedly states, that those who were engaged in occupations connected with animal fats, were most liable to catch the infection of the disease.

But, even though I should not be altogether disposed to discard oil frictions as entirely useless, (for there is certainly some plausible reason for supposing that they may, in some measure, serve to defend the skin from absorbing the noxious matter of the plague), yet there have been so many instances of persons living in the closest cohabitation with the infected, escaping *without the use of oil*, as well as so few clearly attested cases of persons proved to have come into contact with the pestilential virus having been preserved by *oil alone*, that, in my judgment, the inference of its possessing any certain quality as a preservative against the plague is premature, and destitute of any sufficient support. The fact is, many of those who have most diligently used oil frictions have perished; and there are unfortunately several instances at the present moment in the third garrison battalion, of men being assailed by the plague, who, before mounting their guard, (which was the only time they could possibly be exposed to the contagion) were obliged to apply the frictions of oil with all the punctuality of military discipline. Moreover, an immense proportion of the attendants of the sick in the Maltese pest-hospitals, who were enjoined the use of inunctions, have fallen sacrifices to the disease.

But to reason a little upon the point, how shall we have cause to credit that the application of oil frictions to the surface of the

body is entitled to much confidence as a preventive, when we know that it is almost invariably attended immediately after with profuse perspiration? For as the perspirable matter can make its way outward through the excretory pores of the skin with so much facility, is it more unphilosophic to suppose, that when the perspiration has ceased, the subtile virulent matter of pestilential infection may make its way with equal facility inwards through the absorbent pores, at least sufficiently so as to exert its noxious effects upon the constitution? Without meaning to reason conclusively, I advance this as mere matter of speculation.

Those who have perused the account of the great plague in London, cannot have forgot the high repute in which tar, pitch, and some other substances were held as defences against the contagion. Indeed, as a farther exemplification of how far imagination is apt to impose upon mankind in these matters, almost every writer, from the time of Procopius to the present, supplies some instance or other of certain things possessing conservative virtues in this disorder. Among others issues obtained the most unqualified credit. Girolami Mercuriale, whose authority as a writer was in no mean estimation for a considerable time among the Italians, when observing upon the efficacy of issues, uses these strong words: "Dicam quod ego experientiâ vidi. Possum testari me innumeros hâc peste extinctos vidisse, nec unquam quemquam qui habet cauterium præter unum tantum atque ille erat sacerdos." In this assertion, he is supported by several corroborating testimonies of the same time.

I have dwelt the longer on this subject, as it may be of some consequence that the credulity of mankind is not imposed upon, to place confidence in any supposed means of preservation which may only lead to greater risks. Of all the means which have come to us, recommended either in ancient or modern days, as productive of advantage in securing the safety of persons engaged about the infected, I believe none deserves the same degree of faith, as personal cleanliness; avoiding contact, or using immediate ablution after a foul contact; shunning the breath or the vapour exhaling from the bodies of the sick; ventilation; the fumes of the mineral acids; sustaining the animal spirits by generous living, and by exercise; and lastly, though not less importantly, by the use of oiled dresses, the texture of which is so completely stopped up, as to prevent the passage of the most minute particles of any matter from without. In the dreadful plague, which, almost a century ago, swept off 50,000 inhabitants of Marseilles, it was by these means only, (the mineral acid fumes excepted,) that the faculty of physicians entirely escaped, though engaged throughout the whole of that mortality in the closest

closest intercourse with the infected. The success of the same plan, as pointed out by myself in our military pest-hospital, is a decided proof of the credit which is due to this practice; not one of the attendants having been attacked with the complaint, though in constant habits of handling infected substances, and coming into contact with the sick.

All those whom I know to have felt the pulse of plague patients with the naked hand, have, by nothing more than a prompt ablution, escaped with impunity. They were not so much as provided with oil.

Vaccination, though it some time since obtained great repute as a preventive against the plague, and enlisted even the powers of the press in its praise, has not, upon the present occasion, appeared to justify any degree of faith in its virtues. Several observations have been attentively made, the result shewing, that those who had not undergone the operation escaped in just as great proportion as the persons who had been vaccinated.

I have not noticed such striking effects from the variation of atmospheric temperature on the plague as I was at one time led to expect. I am aware, however, that there are not wanting some who imagine, that a diminution in the numbers of the infected was produced by this cause alone. I have, I think, much more invariably remarked, that a high wind, from whatever quarter it came, exerted much more evident effects in diffusing the contagion. Indeed, this was the subject of very universal observation; the people whom I have conversed with attempting to account for it, by supposing that the particles of contagion might be transported and driven with force from one person to another in the street. Lame as this account of the matter must be admitted to be, it will serve as a confirmation to the justice of the remark, that some increase in the numbers of the sick did very uniformly follow upon occasion of a high wind.

The declension of the malady towards the latter end of the summer and autumn, though ascribed by some to the variation of the atmospheric heat, was the plain and indisputable consequence of the rigorous precautions which then had begun first to be properly enforced. The point I think is proved from the suddenness of this change of measures in diminishing the numbers of the sick, and confining the disease almost entirely to the houses that had been previously infected, whilst the alteration of the thermometer was hardly perceptible.

I shall next proceed to give such an account of the method of cure, as I have had an opportunity of gleaning from persons engaged

engaged in attendance about the sick, and from my own practice.

The indications are,

1. When inflammatory symptoms are violent at the commencement of the disease, to moderate them cautiously.

2. To restrain all inordinate efforts of nature, or to support her when exhausted.

3. To counteract putrescency.

4. To evacuate the morbific matter.

These it is proposed to fulfil, by Evacuants, Tonics, Antiseptics, Blisters, Sudorifics.

The evacuants most esteemed by the native practitioners, are mild laxatives,—as cream of tartar, manna, almond oil, &c. particularly this last. It is only at the very first attack that these opening medicines are usually employed by them, and when the bowels are evidently constricted. A purgative, properly so called, is very rarely ever exhibited by the Maltese in this complaint, unless in extremely plethoric habits, and when there is a great phlogistic diathesis; then the Epsom, or *English*, salt, as they call it, is given in a moderate dose.

Bleeding, either general or local, is held to be pernicious, and has seldom or never been had recourse to by the natives on the present occasion. I have myself, however, made trial of it locally, and in habits when, from the state of the other symptoms, and particularly the affection of the brain, I considered it indicated. In this case, I preferred taking away a few ounces from the temples by leeches. I am far from feeling assured, however, that any benefit was derived from the practice, nor am I certain that it has not paved the way for greater mischief, by increasing the indirect debility. It is at best, in my judgment, but a precarious remedy, and I believe it is considered as such by the almost universal consent of mankind, wherever the practice has had a fair trial. I have been informed by one individual, that in a few cases of blood being taken by him from the arm, it threw up a buffy coat, and the patients recovered. By another I was told, that the blood taken in this manner was in a very dissolved state, yet the case proceeded well. These were, by description, all persons of nearly the same habit, and in the same stage of the disease,—robust and plethoric.

When the affection of the brain is urgent, betokened by high delirium or a very low state of coma, I have applied blisters very generally to the temples, the nape of the neck, over the hinder or fore part of the head, or behind the shoulders. Blistering over the abdomen having acquired some degree of credit, I made trial of it also, but cannot give any decided opinion of its advantage.

vantage. The Maltese practitioners seldom or never apply blisters unless under the buboes, and then only with the intention of promoting suppuration, which they are thought to do. In some rare instances they have put them to the nape of the neck, but without placing any great confidence in their success. Sinapisms have been much more confidently represented to me as effectual, when applied to the soles of the feet, with the view of relieving delirium by revulsion. Mild emetics of ipecacuanha are generally thought beneficial, when administered at the very onset of the distemper; given at a late period, they have not been attended with any success, or perhaps have done positive mischief by determining too much to the brain.

The antispasmodics and tonics most esteemed by the Maltese are, Colombo, Gentian, Peruvian bark, and Serpentaria Virginiana, each given in the form of decoction. These are prescribed at a very early moment, when the state of the brain allows of their exhibition, and after debility has begun to supervene.

The sudorific most in use by the natives, is the spiritus mindereri, given freely with plentiful dilution of lemonade, or other mild beverage, and aided by the free use of elder-flower tea. This latter is a favourite remedy among the Turks. My own practice has been entirely confined to the former. I have not been able to learn that antimonials have had any trial among the Maltese in this disease. Used by our military surgeons, antimonials have obtained rather a favourable character, in the form of James's powder, or the pulvis antimonialis of the London Pharmacopoeia.

Vegetable acid, particularly lemon-juice, was given very freely in common drink by the Maltese, to which practice the Asiatics are likewise much devoted. But I cannot persuade myself that the promiscuous use of these acids is not pernicious, in cases where there is a strong disposition to the generation of acidity in the stomach and bowels.

The actual and potential cautery have each obtained repute for their efficacy in the maturation of buboes. They were resorted to by the native practitioners on occasion of these tumours coming forward very reluctantly, and when, at the same time, the other symptoms proceeded doubtfully or unfavourably. But when the actual cautery did not soon succeed in promoting suppuration, I have been credibly informed, that the tumours have receded, and this recession has been speedily followed by aggravation of all the other symptoms.

When the glandular and other tumours were accompanied with any great degree of pain, emollient poultices gave much relief.

After

After they had degenerated into a sloughing state, warm stimulant applications produced good effects in causing the separation of the dead from the living substance. With the same intention I have seen camphor and oil of turpentine applied with benefit, whilst scarification also materially assisted in expediting this separation.

Camphor, given internally, has long maintained a great share of credit in the treatment of plague, exhibited in moderate doses, being considered useful as a stimulant and antispasmodic, and in certain states of the brain. It has not, however, been much employed by the Maltese. As far as my own observation goes, camphor, like some other medicines, appears to have been extolled for virtues, which experience by no means proves it entitled to.

Calomel has not been in use as a purgative among the native practitioners in the plague. It was in a few instances administered in small doses, as one of those gentlemen informed me, with the intention of co-operating with external mercurial frictions, or applications for the maturation of buboes.

Opium is supposed in some cases to be good, when there is much restlessness, and provided the condition of the brain and other symptoms does not contraindicate its employment. I have myself not given it a trial. When it succeeds in composing the patient, and in favouring perspiration, it becomes a valuable remedy: but it is found necessary to use it with great circumspection.

Wine is not in very general use with the Maltese for the treatment of any stage of the disorder. In my own practice, I could ascribe much advantage to its cautious exhibition, at an advanced period, particularly when there was much debility and low delirium. I generally directed it to be given in sago, and in moderate quantity. When wine was employed by the natives, the sweet malaga was preferred.

Cordials were resorted to with equal caution as wine.

Some cases were detailed to me by the surgeon of the 3d garrison battalion, in which he thinks he succeeded in arresting the progress of the plague, by the very early and diligent application of mercurial frictions, giving previously not less than 12 grains of calomel, in robust habits, to clear the bowels. Of these cases, this gentleman informed me he had not kept a detail, but, in most of them, besides other suspicious symptoms, he said there was the evidence of tumefied glands. The patients rubbed in the ointment themselves. Bleeding, either general or local, was not employed but in one or two of these cases. The warm

warm bath was tried with a few, and seemed to be of some advantage in assisting the operation of emetics.

I have tried the cold affusion in two cases, in one of which it appeared to produce very immediate good effects. The patient was soon relieved in all his feelings, and there succeeded a perspiration, aided by sudorifics and diluents, which in a short time put to flight all the symptoms. The cold water was applied about the third day after his confinement. Along with the remission of other symptoms, the bubo disappeared. This was a case in every respect such as Dr Currie describes as adapted to the experiment in typhus. In the second instance, I had not the same reason to hope for its good effects, on account of the length of time that passed previously to its employment, and the exhausted condition of the patient.

A Maltese practitioner detailed to me one very striking instance of the good effects derivable from the sudden application of cold water. The case was that of a man who, in the heighth of delirium, and labouring under a most unpromising disease, ran violently out of his chamber, and precipitated himself into the sea, from which he seemed to have experienced the most sudden relief. He repeated this a second time of his own accord, and was soon restored to a state of convalescence, from which he perfectly regained his usual good health. This case, too, was one in all respects, from the description I received of it, well suited to the experiment of the cold affusion. The application of cold water to the surface, has had no trial in the Maltese pest-hospitals.

The theriaca continues in some estimation among the Maltese, as a sudorific and febrifuge, and was usually given in the evening.

The Maltese have been under the same restrictions in feeling pulses in plague cases as the English. They consider it themselves as altogether an unprofitable risk among the means of judging of the patient's situation, regarding the state of the other functions as a sufficient direction for every purpose of practice.

Such is nearly the sum of the information I have been enabled to collect upon this disheartening subject; the result of which, I deeply lament to see, goes but to verify the words of the poet:

> Dum visum mortale malum tantæque latebat
> Causa nocens cladis, pugnatum est arte medendi,
> Exitium superabat opem, quæ victa jacebat.

So true is the observation of the celebrated physician, Chicoyneau, that there is no disease in which so many modes of treatment have been devised without effect.

With

With respect to any point which I have here treated upon the representation of others, I shall hope for every due forbearance, should I have failed, in any respect, in my intention of fully and exactly producing the meaning of my authority. I might have noticed many other opinions, but conceive that what are already stated embody the sum nearly of what is most important to be dwelt upon with any earnestness. In a word, I believe it may be roundly asserted, that, after all this fatal experience, not one single remedy has been stumbled upon in which any certain efficacy was discovered to reside, and that the practice in plague is still at the present day little more than an empirical routine.

No one more sincerely laments than myself, the restrictions which were laid upon scientific investigation, to explore the nature of this destructive disease, particularly as so fair an opportunity of obtaining some useful knowledge respecting it may perhaps never again offer to a British practitioner.

It is pretty evident, from the crude detail I have succeeded in gleaning of the notions entertained by the practitioners of this island, respecting the treatment of the plague, that they still adhere to the old opinion of its belonging to the class of putrid typhus, and that their practice, with the exception of bleeding, is not far different from that recommended by Sir John Pringle in malignant fever.

It will be easily believed, that, the dissection of dead bodies was a privilege quite out of the question, since the mulct for feeling a pulse, even through a tobacco leaf, and with every possible caution, was not less than 15 or 20 days close quarantine.

The principal navy-surgeon of this station was confined to his house for twenty-five days, on account of having felt the pulse of a man in his own particular department, and though, after the contact, he used every kind of care to prevent the absorption of the virus. This gentleman, Mr Allan, was one of the most zealous advocates for carrying the investigations of science to unravel the mysterious nature of this distemper, whose hindrance in so laudable a purpose is the more to be lamented.

It may be proper to notice, that His Excellency Lieutenant-General Oakes, whilst opposing my desire of visiting the Maltese pest-hospital, was pleased to assign as his motive for so doing, an anxiety that the army might not be deprived of medical assistance in the time of need. Could I have myself ventured an opinion upon the subject, in opposition to so high authority, I should certainly not have hesitated to say, that this prohibition of medical officers from visiting the sick, so far from being an eligible measure, was calculated to oppose the most complete impediment against availing the army of any benefit whatever from their advice, by shutting out the only satisfactory inlets to medical

medical knowledge. If this be not admitted, it must follow, I should think, that it was of very little consequence whether medical men were employed or not.

It may be expected that I should say something on the history and introduction of the plague into the island. The most current account is, that of its being brought in by a vessel from Alexandria, laden with linen, flax, and leather. But as there were so many channels through which, from the state of the quarantine defence, this insidious malady may be supposed to have crept in, it appears to me little less than mere waste of time, to take any pains in tracing this event to any particular instance of laxity in the department of public health. It is somewhat remarkable, that the history of the introduction of the plague, when it made so great ravages on the last occasion in this island, above a century ago, was nearly similar to what is circulated of the present, being attributed to some linen brought from a Levant vessel by a Maltese shopkeeper, which, after producing the disease in all those who first came in contact with it, ultimately disseminated the malady through the whole population. The same delay, too, which proved so fatal in the present instance, was at that time also the cause of the extension of the disorder. Whilst the different colleges on the continent of Europe were referred to for their opinions on the identity of the disease, it was daily gaining ground, and when finally the answers of those colleges arrived, deciding upon the nature of it, a lamentable proportion of the population had already been carried off. So necessary is it to oppose with vigour the first advances of this deadly enemy, and by the earliest moment to be prepared against his attempts. The faculty of Rome, addressing a Maltese physician at that time on the precautions to be enforced for the public safety, emphatically observes, "Denique licet aliqui negent eum morbum pestis contagiosæ nomine insigniendum, tamen in casu ancipite ubi de publica salute agitur, tutior pars eligenda etiam cum incommodo rerum, ne serpet latius malum." This is the doctrine which was so unfortunately thrown away upon ourselves at the present time. It would not, perhaps, be strictly in place, to produce here any detail of examples to prove the truth of this, else I might, even from my own limited experience of the proceedings of that department, fill a great many pages upon this topic alone. To pass over every other instance, I may merely observe, that the servants who conducted the first case of plague to the Lazaretto returned in eight days, and mixed at liberty with the population; that the physician who attended this case was likewise left at liberty to go through the city before being subjected to any quarantine re-

strictions; that the public hired carriages were allowed to take fares, though lined with cotton; that, for several weeks after the plague entered Valetta, the intercourse among the population was more or less promiscuous, the seclusion of people within their houses not being enforced with any punctuality; that even the carter who drove a pest-cart was not put under quarantine restrictions for a considerable length of time, in consequence of which he was in the frequent habit of going out at pleasure, and marketing for his family, the greater part of whom died of the plague. Since so many instances existed of laxity in the department of public health, can it be wondered that the plague had a rapid and wide circulation? Nay, until the month of July was well advanced, there was not even yet a corps of *trusty* guards organized to give proper effect to any ordinances of public health. The houses of the infected inhabitants were shut up for weeks, without being purified or cleansed, although containing articles of the most susceptible kind, and even living animals, whose escape, it is well known, is liable to carry the disease wherever they went. But, without troubling the reader with any farther enumeration of such instances, the following public advertisement, under the auspices of the Board of Health, will itself sufficiently exhibit the description of *obedience* which was exacted to the regulations of that body. I quote an extract from this advertisement, in the exact terms in which it was printed: viz.

"*Advertisement.*

"Many of the inhabitants who *may be desirous* of taking every precaution against the scourge of plague, are unacquainted with the necessary measures which should be adopted:" here followed a number of recommendatory rules, and among others, to avoid eating *pork*, and concluding with observing, that "the chief of every family who *wishes* to adopt these precautions, must announce his *intention* to his dependants, who not consenting to conform thereto under the apprehension of the rigorous punishments prescribed for the violation of the quarantine laws, will be allowed to quit the house.

"*Board of Health*, 12*th May* 1813. J. Thomas, President."

Thus the grand punishment for violating any of the quarantine laws, was no more than that the delinquent should "be allowed to quit his house;" get the disease, of course, and carry it about to his neighbour! What despondent reflections must not such an appearance of blindness to our danger have been calculated to raise in the mind of any one duly apprised of the consequences? I confess I saw it with horror and impatience, which caused

caused me to take pains in pointing out my sentiments freely, and with an earnestness, which, considering the manner in which the offer of my advice was received, nothing but a strict and conscientious sense of my duty could have prevailed with me to do.

I trust it will never be seriously attempted to impute to the Maltese any positive disposition to oppose the laws of the health department, for, in justice to these worthy and unoffending people, as far as my own experience has gone, I never witnessed so much truly Christian patience, as they evinced in every instance of compliance with the restrictions imposed upon them; that is, after these restrictions did really come to them in the full shape and authority of law. But until an adequate sanction was provided to give the law the necessary efficacy, we cannot be surprised that the multitude, ignorant of their danger, should not have been, of their own accord, very rigid in the observance of any rules or regulations, which, from the manner of their being executed, it is possible they might have thought were not intended to be very religiously obeyed.

The following few remarks, the result of about half an hour's visit to the Maltese pest-hospital, on the 2d of June, may convey some faint idea of the sufferings and privations to which those labouring under this horrible disease were subjected. These miserable creatures lay within a very short distance of each other, five or six on the floor of the same room; 28 of them were attended by two convicts. They had no change of linen, and were therefore obliged to lie, either without shirts, or in their foul every-day clothes. As my object in requesting permission to visit this hospital was to obtain some useful knowledge of the disease, and its treatment, I was prepared with a series of interrogations to put to the medical men whom I accompanied thither. It will not be supposed that my curiosity was likely to be very satisfactorily gratified, when I was told that there was neither book nor register, of any description, in which the details of treatment, or any history of the cases were noted. The small rooms in which the sick lay not being purified and whitewashed before fresh cases were put into them, became, in consequence, mere hot-beds of fomites.

The report of a very respectable and intelligent practitioner, Mr Saunders, late of the royal navy, who, like myself, was allowed to visit this hospital only once, on the ground that the patients might not be disturbed in their last moments, is as nearly as possible coincident with the above. This gentleman farther states having seen both sexes lying in the torments of the disease, in the same apartment, and having asked likewise for some account

account of the medical treatment, was very little more fortunate than myself, being referred for his information to a piece of board exposed at the outside of the pest-house door, on one side of which was written in chalk the medicine for the day (or spiritus mindereri), and on the other, the medicine for the night (the theriaca), without any regard to individual peculiarities, age, sex, temperament, habit, condition of the symptoms, or any other particular of the sufferers' state.

Those who have witnessed the scenes of misery occasioned by the disease among the poor class of Maltese, must the more devoutly render their thanksgiving to God for his merciful forbearance to themselves. Whole families have been literally swept off in the space of a few days; and when a sufferer escaped the common fate of his countrymen, it was but to witness the destruction of any little property he had been possessed of. Bereft perhaps of every relative, and every friend he held dear, his return to a desolated abode was all the consolation he had to compensate for the loss of every thing else which could render life desirable.

The proportion of recoveries among the Maltese has been variously stated, but I believe the most authentic account does not place it above one in ten, or thereabouts, on an average. At the beginning few or none escaped, but towards the wane of the distemper, the cases became fewer and milder.

Amidst this scene of calamity, it must afford very inexpressible gratification to know how few of our countrymen have fallen sacrifices to the disease, and that the army has been so far fortunately preserved. The inheritance of a sound constitution and difference in mode of living may probably have very much contributed to confer this greater degree of security. Every credit is certainly due to commanding-officers, for their early attention to some of the most necessary precautions. The 14th regiment and artillery enjoyed peculiar advantages in the situation of their barracks, which shut them out completely from all possible communication with the population. The garrison battalion and De Roll's lost the most men, which can only be ascribed to their being placed in situations of greater exposure. In all, our army has not hitherto lost above 20. I have not, however, seen the returns of the garrison battalion, since it has become a second time infected.

Before I conclude this paper, it is proper to observe, that the information I have collected from the native practitioners, is much more the result of their speculation than practice. Their treatment of the plague, according to every account I have received of it, is very simple and summary. With perhaps every inclination

inclination to pursue inquiry, and to assist their fellow-mortals, they had certainly but few facilities to encourage them in either the one or other of these offices. Patients were not placed under their care, in the greater number of instances, before the disease was advanced, and when the golden moments for rendering them any assistance had irrevocably passed. As there is no disease in which the maxim of *venienti occurrite morbo* is of such important application as in this, the consequences of delay in sending patients to hospital must have been inevitable.

Not having myself any concern with the department of public health, it is not in my power to furnish any of the numerous public papers relating to the progress of the calamity. This task must therefore devolve upon those whose connection with that department, or better opportunities, have procured access to these papers. It may, doubtless, be matter of much curiosity to peruse such documents; but from all I have myself seen and learnt, I do not flatter myself with much expectation, that the sum of our knowledge, either upon the nature of the disease, or the manner of treating it, is likely to derive from them any materially useful augmentation.

The deaths occasioned by plague in the several months, are as I have below copied them from the printed notices, viz.

Abstract

Abstract of the number taken ill and dead of the Plague in Malta, 1813, as copied from the printed notices.

Month.	Dates.	Died.	Sick.	Total Died.
April	1	1		1
May	3	1		
	7	2	1	
	12	1		
	16	3	1	
	17		1	
	18	2	5	
	19	8	8	
	20	10	11	
	21	6	5	
	22	6	5	
	23	4	3	
	24	7	3	
	25	4	4	
	26	5	10	
	27	5	12	
	28	7	10	
	29	14	12	
	30	10	15	
	31	12	16	
June	1	18	11	109
	2	8	13	
	3	15	17	
	4	10	11	
	5	16	31	
	6	24	22	
	7	19	14	
	8	28	11	
	9	24	26	
	10	32	33	
	11	27	36	
	12	38	40	
	13	36	14	
	14	86	17	
	15	19	15	
	16	19	16	
	17	23	30	
	18	28	25	
	19	21	24	
	20	13	22	
	21	24	34	
	22	32	19	
	23	29	30	
	24	39	44	
	25	23	36	
	26	34	56	
	27	39	39	
	28	37	46	
	29	44	50	
	30	47	48	804
July	1	53	44	
	2	42	43	
	3	44	34	
	4	53	46	
	5	43	53	
	6	49	57	
	7	58	54	
	8	54	86	
	9	51	51	
	10	52	30	
	11	56	56	
	12	68	60	
	13	59	54	
	14	55	61	
	15	65	30	
	16	67	30	
	17	36	37	
	18	50	47	
	19	41	20	
	20	43	47	
	21	41	41	
	22	55	30	
	23	47	37	
	24	54	43	
	25	49	41	
	26	53	51	
	27	47	63	
	28	64	36	
	29	51	50	
	30	58	49	1596
Aug.	1	53	41	
	2	64	27	
	3	48	41	
	4	27	39	
	5	21	20	
	6	13	24	
	7	33	20	
	8	35	20	
	9	28	24	
	10	26	28	
	11	28	21	
	12	26	26	
	13	31	26	
	14	31	29	
	15	26	39	
	16	34	28	
	17	29	28	
	18	32	28	
	19	38	45	
	20	27	27	
	21	33	29	
	22	32	28	
	23	34	17	
	24	33	23	
	25	33	30	
	26	31	11	
	27	38	18	
	28	27	17	
	29	25	25	
	30	29	15	1023
Sept.	1	26	32	
	2	37	13	
	3	33	53	
	4	32	23	
	5	28	15	
	6	33	33	
	7	34	32	
	8	37	14	
	9	31	15	
	10	29	14	
	11	25	22	
	12	17	14	
	13	26	7	
	14	17	18	
	15	17	14	
	16	30	22	
	17	23	8	
	18	12	20	
	19	20	16	
	20	24	19	
	21	21	9	
	22	12	7	
	23	12	6	
	24	16	12	
	25	11	14	
	26	11	9	
	27	19	10	
	28	12	19	
	29	15	13	
	30	14	9	675
Oct.	1	16	11	
	2	12	11	
	3	16	10	
	4	10	5	
	5	7	9	
	6	6	9	
	7	14	9	
	8	13	1	
	9	9	3	
	10	8	3	
	11	5	4	
	12	3	8	
	13	5	5	
	14	4	2	
	15	3	3	
	16	5	5	
	17	5	3	141
				3348

Month.	Total Died.
April	1
June	804
July	1596
Aug.	1023
Sept.	675
Oct.	141
Total Gen.	3348

The average deaths per day being now so very inconsiderable, I would indulge every hope, that a due perseverance in the necessary means of caution may soon finally put a period to our alarms. My only apprehensions arise from the negligence which may take place in the expurgation of infected houses, and the destruction of susceptible articles liable to retain the contagion.

Brief Memorandums of the Treatment of the patients in De Roll's regiment, under charge of Dr Faulkner, Physician to the Forces, in the military pest-hospital, Malta, 1813.

Daniel Boan, æt. 22, a very stout man. Admitted into the pest-hospital, 4th July. Was taken ill on the 3d. Previous to removal out of his barracks, complained of headach, and want of appetite: tongue white; some appearance of inflammation below the inguinal glands, on one thigh, attended with stiffness; sleeps ill; belly costive; pulse accelerated; eyes suffused. Had an emetic of ipecacuanha and tartrite of antimony, before removal out of the barracks, and ʒij. of the aq. am. acetat. every three hours; barley-water acidulated with lemon-juice, for common drink, and a mixture of cream of tartar to open his bowels. The skin, which was hot and dry, was directed to be sponged with vinegar and water frequently. Ventilation and fumigation with the nitrous gas were kept up in his room, constantly after his admission into the pest-hospital.

Die 5.—Bubo, (described above as an inflammation below the inguinal glands) more painful; a great appearance of distress in his looks.

Si venter non solvitur, hac sera habeat pulv. rhab. gr. xx. et calom. gr. iv. Perstet in aliis.

Vespere.—Bowels still bound; pulse quick; appetite still much impaired; thirst considerable; has had no sleep; was subjected to the cold affusion this forenoon, from which he stated he felt himself greatly relieved; still a great look of distress in his countenance.

Appr. vesicatoria abdom. inferiori et temporibus; item hirudines iv. temporibus. Perstet in aliis.

Die 6.—Has had two stools in the night; perspires freely ever since the affusion; thirst more moderate; slept better; appetite still deficient; on the whole, much improved.

Habeat vinum rubr. Port dict. cum sago mixtum pro diæta. Capt. spt. æther. vitriol. ʒss. bis die. Acid. muriat. gtt. x. quater in die. Appr. cataplasma emolliens buboni. P. in aliis.

Vespere.—Seems to improve; has had no stool since last night. Mane habeat solutionem supertartrit. potass. ut supra.

Repr. cataplasma emolliens. Habeat aq. ammon. acetat. ʒij. tertiis horis. Continued the rubbing his body with tepid vinegar and

and water: barley-water freely given for common drink, acidulated with a very small quantity of lemon-juice

Die 7.—Much improved; swelling and inflammation of femoral glands disappearing. Applied a discutient lotion to the bubo; was directed a glass of red port wine three times a day, and to go on with his other medicines.

Was removed into another chamber, after being thoroughly washed and shifted. Under the use of the remedies, he continued convalescent, and in a few days was gradually restored to his original health.

ANDRICK, æt. 38, of a weakly constitution.—Received into the pest-house on the evening of the 8th July, about six o'clock, when he could give no account of himself. No case was sent by the surgeon of his regiment.

Soon after admission, had an emetic of ipecacuanha and antimony; was directed to have sago and beef-tea for diet, and acidulated barley-water for common drink.

Die 9.—Continued in the same state, and senseless as on admission; has a large swelling in the femoral glands of one thigh; bowels rather purged. Port wine was added to his sago, in very small quantities. He was directed to take a table-spoonfull or two occasionally, of the M. cretacea of the L. P. and ten drops of muriatic acid three times a-day The body was ordered to be frequently sponged with vinegar and spirit of wine, and a repellant lotion to be applied frequently to the bubo.

Vespere.—Symptoms aggravated, and the state of coma more fixed; purges much; has a very pale cadaverous look. The lemon-juice was ordered to be left out of his common drink, and blisters to be applied to his temples and lower belly.

Die 10.—Nearly in the same state; is still much purged; about 30 drops of laudanum were added to a seven ounce and half mixture of the M. cretacea, with a little aromatic confection, and of this, two table-spoonfulls were directed three or four times a-day. The small quantity of port wine and muriatic acid were likewise continued as before, and an emollient cataplasm put to the bubo.

Vespere.—Little change; if any difference, he appears rather easier. Perst.

Die 11.—This morning there is a great depression in his looks; could not give any account of his sufferings, but seems to point at a pain in the region of his stomach and belly, which causes his uneasiness. Passed two worms of the round kind. Hot fomentations were immediately ordered to be applied, with directions to omit the wine and chalk mixture, should the pain continue. In an hour or two afterwards, I found him *in articulo mortis.*

PETER WEBBER, æt. 43, a stout man. July 20th.—Admitted into the pest-hospital, at six o'clock, P. M. The following case was sent me of this man by the surgeon attached to the regiment.

"July

"July 20th, 5 min. before 12 o'clock.—Complains of headach, and has two evident swellings on both thighs, below the groin; tongue foul; has some appearance of stupor in his countenance. At the examination yesterday evening, and this morning, there was nothing observable in this man to lead to a suspicion of his having the *plague*." He had no stool for two days previous to his attack. When he reported himself unwell with the above symptoms, he would not admit that there was any pain or uneasiness in the swellings under his groins, but complained very much of thirst. Leeches were directed to be put to his temples, but they could not be had in time, before his removal to the pest-hospital. Was directed an opening mixture of cream of tartar; small doses of the aq. am. acetat. and acidulated barley-water for common drink.

On admission into the pest-hospital, all the symptoms were greatly aggravated; leeches were immediately ordered to be put to his temples, and soon after blisters.

Die 21.—Great delirium, and wild staring look of the eyes, accompanied with voracious appetite, manifested in the avidity with which I found him devouring a piece of bread given to him by one of the attendants; is much purged; the leeches had drawn but little blood; a cataplasm was immediately applied to the buboes, and a trial given to the cold affusion.

The following directions were left with the medical attendant:

Habeat acid. mur. gtt. x. quater in die. Omittr. mist. supertart. potassæ, et si purgatio continuat. habeat mist. cretaceam sæpius. Post affusionem appr. vesicatorium abdom. inferiori. Diæta sit jusculum tenue, sago, et aq. hordei.

Hora prima, P. M.—Has continued all the morning in a most furious delirum; tore off his blisters; not purged at present. Omitt. mist. cretacea. No particular effect produced by the affusion.

Adde aq. hord. lb. i. nitri purificat. ʒss. pro potu. Continued in the same state until his death, at 12 o'clock at night.

Yaniwicz, about 30 years old, very healthy and vigorous.—Admitted in the pest-hospital, on the evening of the 21st July, at half past seven o'clock, with buboes and a carbuncle situated inside one of the glutæus muscles.

From the case sent by the surgeon, it appears this man had buboes in each groin, and a slight diffused swelling in the lower glands of the left thigh, red and inflamed; had symptoms, as the surgeon thought, of gonorrhœa, which he discovered four days previously, but concealed; was in hospital two years ago, with venereal complaint, since which, he affirms, he has not exposed himself in any way to that disease.

At entrance into the pest-house, was ordered beef-tea and sago, with acidulated barley-water, for common drink. Two grains of calomel were likewise directed night and morning, and ʒij. of the aq. am. acetat. every third hour. The medical attendant resident at

at the pest-hospital, was directed conditionally, if the symptoms should grow worse, to give an emetic of ipecacuanha and antimony, and to apply blisters to the lower belly, smeared with mercurial ointment, and to apply emollient poultices to the buboes. The blisters were directed to be smeared, proceeding on a motive, which obtained some credit among the Maltese, that a salivation would be more readily produced.

Die 22.—Says he feels more comfortable, and that the pain of the buboes is not so great; tongue cleaner; not so much thirst; the blister was not applied. Adhibeatur frictio lenis cum aceto et alcoholi per corpus sæpius in die. Perstet in aliis.

Vespere.—No visible alteration. Perstet.

Die 23.—Represents himself as better; a wild intoxicated cast of countenance, yet without any incoherency in speech. Blisters were applied to his temples, and to the nape of his neck, and a glass of port wine given to him three times a-day.

Vespere.—The carbuncle much inflamed. P.

Die 24.—Lost about five ounces of blood by the leeches; was delirious in the night; had four stools yesterday. Appr. vesicatorium abdomini inferiori cum hydrarg. unguent. fort. illitum. Omittr. jusculum et vinum.

Vespere.—Has had three stools in the course of the day; considerably delirious. Barley-water was ordered to be given abundantly for common beverage, and a blister to be put to the fore-part of his head.

Die 25.—Very delirious in the night; broke the windows of his chamber; has had four stools since yesterday; does not complain of thirst; tongue cleaner. Perst.

Vespere.—Much purged. Omittr. aq. am. acetat. et habeat mist. cretac. ℥j. ter die. Item camphoræ gr. v. ter die in oleo olivæ solut.

Die 26.—Has had four stools to-day; is very incoherent at present; was extremely delirious in the night; in other respects nearly the same; has torn off all his blisters. Perstet.

Vespere.—Is nearly in the same state as this morning. Perstet.

Die 27th.—Is not quite so much purged this morning; continues in the same state of delirium, and will not allow the blisters to remain on. Perstet.

Vespere.—Seems rather more distinct; has had but one or two stools to-day. Perst.

Die 28.—Is able to give some account of himself; was however delirious in the night. Perstet.

Die 29.—Seems a good deal improved; carbuncle in a state of sphacelus; belly almost natural. Habeat cinchon. pulv. ℥ij. ter in die; acid. muriat. gtt. x. ter die.

Vespere.—Goes on to amend. Perstet.

Die 30.—Nearly in the same state; the carbuncle was scarified, and digestive ointment applied to it.

Vespere.—No alteration in the symptoms.

Die

Die 31.—Complains of being much purged last night, which, on being questioned, he ascribed to some camphor; carbuncle sloughing. The five grains of camphor omitted.

Die Augusti 1.—Not quite so violently purged. On looking accidentally into the hospital-mate's prescription book, I observed that ʒj. of camphor, and ʒiv. of oil of turpentine had been directed for the carbuncle, which, upon inquiry, I found the attendant had administered on the 30th ult. *internally!* This will fully account for the purging on the 30th and 31st of that month.

Vespere.—The purging is moderated; in other respects seems in a favourable state. Perstet, et habeat sago pro diæta.

Die 2.—Purging nearly ceased; says he slept well last night. Perstet in medicamentis ut heri; was conditionally ordered a drachm of bark three times a-day, provided the purging did not threaten.

This treatment was persevered in for a few days, and he became completely convalescent.

Drummer Gisler, æt. 21, very stout.

The following case of this man was sent by the surgeon:

July 23d.—"A tumour apparent in the inferior inguinal glands of the right thigh, accompanied with considerable pain; says he felt the pain at last evening's parade; headach, which is better since the morning; tongue rather foul."

Removed to the pest-hospital at Floriana, about half past seven o'clock P. M.

At admission into the pest-hospital, was given eight grains of calomel, and 15 of jalap in my absence; had plain barley-water for common drink.

Die 24.—Had three stools in the course of the night; countenance much flushed; six leeches were immediately ordered to the temples, and about two drachms of the aq. am. acetat. every third hour.

Vespere.—About half past three o'clock rushed out of his room in a high delirium; only about six ounces of blood could be taken away by the leeches; is much purged; a blister was ordered to the nape of the neck; and, if the delirium should increase, one also to the forehead.

Die 25.—Very much purged; high delirium; great thirst; tongue white; a blister was directed to be applied over the crown of his head; and five grains of camphor, dissolved in olive oil, to be given three times a-day; an emollient cataplasm was put to the bubo; and he took a small quantity of the chalk mixture to stop the purging.

Vespere.—Has had five stools to-day; bubo very large; complains of great pain in the head; the blister was not applied to the head yesterday.

Die 26.—Died in convulsions, about eight o'clock, A. M.

Odin,

Obin, æt. 37, of a weakly habit.

The following is the case of the patient, as furnished by the assistant-surgeon.

25th July 1813, one o'clock, P. M.—" Complains of headach, thirst, which he says were preceded by shivering; ascribes his illness to the cold air of the guard-room; slept ill on the night preceding; skin hot; eyes rather suffused; tongue foul; has had no stool to-day; has taken an ounce of Epsom salt, which he vomited almost immediately, and without producing any effect in opening his bowels; states he had intermittent fever in Sicily a year ago; about four o'clock there was an appearance of redness in the left groin, but without any visible swelling; had an emetic about half past four o'clock.

" Die 26, horâ 6tâ, A. M.—Is considerably better; does not complain of any pain in the groin, but there is now an evident swelling where the redness had appeared, attended with inflammation; tongue rather foul; seems very feeble; removed to the pest-house at Floriana 12 o'clock this forenoon."

Horâ 12. M.—This patient walked to the hospital, refusing to be carried on a hand-bier, of which he expressed a great horror. Since his admission into the hospital, appears very ill; has had an opening mixture of cream of tartar, his bowels not having been free for some days. Two grains of calomel, and two of antimonial powder, were directed to be given every four hours. Ordered ʒij. of the aq. am. acetat. every third hour.

Vespere.—continues in the same state. Perst.

Die 27.—Has had two stools to-day; complains of great thirst; has been in a state of low delirium since yesterday, after admission into the pest-hospital; is very much debilitated. Perstet in medicamentis; habeat jusculum tenue pro diæta.

Vespere.—Is nearly the same as this morning. Habeat camph. gr. v. ter die. The attendant says he has passed something like corrupted matter, mixed with blood, in his stools.

Die 28.—Died this morning at half past six o'clock. The whole body was of a bluish cast after death; the bubo never tended to suppurate. The same state of low delirium that succeeded his admission into the pest-hospital, continued until death. He never mentioned any particular complaint, or expressed the presence of any pain.

Konoblezky, æt. 26, stout.

The following case was furnished by the surgeon:

25d July.—" Has an evident florid tumour, first discovered yesterday evening, in the inferior inguinal glands of the left thigh, and a carbuncle, situated in the middle of the tibia. He complained also of headach this morning, which has been relieved by bleeding from the nose; tongue rather foul; symptoms continued much the same when he was removed to the pest-hospital at Floriana, about half past 12 o'clock, M."

Die

Die 23.—On admission into the hospital, the following directions were given:

To apply a repellent lotion to the bubo; to give ʒij. of the aq. am. acetat. every three hours; and two grains of calomel night and morning; to drink copiously of barley-water; and to confine himself entirely to weak beef-tea for ordinary diet.

Die 24.—Had two stools this morning; tongue not very foul; is not much altered from the state he was in yesterday. An emollient poultice was directed to the carbuncle; and the other medicines to be continued.

Vespere.—Two passages by the belly since last night; same appearances of countenance. Abrad. capillitium et appr. vesicatorium occipiti; omitt. jusculum; perst. in aliis; capt. camphoræ, gr. v. ter die.

Die 26.—In the same state nearly. Last night he ran out of the hospital in a high delirium. Had four stools to-day.

Vespere.—Perstet.

Die 27.—Has torn off all his blisters, and continues still very delirious. Perst. ut antea.

Vespere.—Not so well. P.

Die 28.—Seems rather more composed and distinct this morning, and was able to communicate, that he slept some in the night, and had two stools. Habeat acidi muriat. gtt. x. quater in die. Has a look of much anxiety and depression. Habeat vin. rubr. cyathum ter in die.

Die 29.—No change. Perst.

Die 30.—Carbuncle has begun to slough; some digestive ointment was ordered to be applied to it. Appearance of countenance more settled.

Die 31.—Most severely purged in the night. On looking into the prescription-book of the hospital-mate, I observed the same application for the carbuncle as ordered for Yaniwicz above; 4 ounces of oil of turpentine and ʒj. of camphor.

Vespere.—Continues still much purged, and complains of great griping and uneasiness about the belly.

Die Augusti 1.—I found the same mistake had been committed as in the case of Yaniwicz, and that the oil of turpentine and camphor had been given internally.

Die 2.—The purging moderated, and he feels much easier. Cont. medicamenta ut heri. Habeat cinchonæ ʒi. ter die; omittendo tamen cinchonam et vinum, si purgatio minitat, vel dolor abdominis.

Vespere.—Ulcer more healthy; bubo opened this morning, and discharges freely. These medicines were persevered in, and, in the course of a few days, he was restored to a perfect state of convalescence.

Nicola Seiler, æt. 26, of a vigorous constitution. The following case was furnished of this man's disease:—

11 o'clock A. M. 28th July 1813.—"Complains of headach, thirst, want of appetite, an inclination to vomit, and pain in the limbs; says he felt headach yesterday evening, and vomited this morning. Tongue a little foul; skin hot; had an emetic of ipecacuanha and antimony, and a large quantity of rice water for beverage.

"29th, morning.—Headach and thirst increased; no stool yesterday; complains of a pain in the right axilla; but there is no perceptible swelling; had leeches to his temples; and took an ounce of Epsom salt; succeeded in obtaining two dejections, and about 10 or 12 ounces of blood.

"At half-past 8 o'clock.—Says he feels no pain of head or any other part of the body, the axilla excepted, in which there is a very slight swelling. Habeat aq. am. acetat. Put a stop to the hæmorrhage, (which it was found difficult to do from the largeness of the leech bites.) Repeated the aq. am. acetat.

"30th July, about 5 o'clock, A. M.—Said he had slept well; had no headach; scarcely any pain in the axilla, and none in any other part of the body. Swelling in the axilla not increased, and he feels much better in every other respect; tongue white.

"8 o'clock.—Continues the same."

Thus far proceeded the surgeon's report.

At my visit some short time after this last report, I found the patient in a state of low delirium, and the swelling in the arm-pit increased. Immediately blisters were directed to his temples, which he tore off almost as soon as they were applied. The other medicines were continued. He died in the course of the evening.

After death he was covered with dark-coloured spots of about the size of a farthing each.

Shortly before his decease, about 10 gr. of camphor were administered.

Bregati, æt. 36, of a weakly habit. The surgeon supplied me with the following case of this patient:

August 2d 1813, half-past 6 o'clock A. M.—"Complains of headach, nausea, and some thirst; skin hot; tongue rather foul. Has a carbuncle on the outside of the left leg, and some pain in the left groin, but there is no evident swelling. Has been subject to attacks of fever formerly.

R. Pulv. ipecac. gr. xv.

"10 o'clock.—Emetic operated, but does not seem to have produced any other immediate effect; much dejection, and apparently confusion. At 1 o'clock P. M. applied leeches to his temples, and took the aq. am. acetat. Headach still continuing, about 4 o'clock applied a blister to the temples. Removed to the pest-hospital at Floriana at a quarter before 6 o'clock P. M."

Thus far the case as given by the surgeon.

At my visit to this man about 4 o'clock P. M., in company with the deputy-inspector of hospitals, it was agreed by us, that there was some appearance of swelling on the left thigh, where he complained of

of having pain. The leeches took away not more than about five ounces of blood.

After the patient's admission into the pest-house, his complaints were of headach, thirst, and debility; he had had no stool since the morning; much appearance of distress and wildness in his looks; tongue foul.

The following directions were given, viz. that he should have plenty of plain barley-water for common drink; that he should have two grains of calomel and two of the pulvis antimonialis every fourth hour; and that his body should be frequently sponged with vinegar and spirit of wine.

Die 3.—Wildness of countenance the same, complains of great headach; face flushed; slept very ill last night; no appetite; great thirst; no stool since yesterday. A blister was directed to be applied without delay to his temples and nape of the neck; and an ounce of Epsom salt to be given, with copious dilution, continuing the other medicines.

Vespere.—Had one trifling evacuation by his bowels about half-past 3 o'clock P. M. Complains of some pain in the belly; tongue not very foul; seems restless and very confused. A blister was ordered to the lower belly; and a clyster to be thrown up. Instead of the calomel and antimonial powder, ʒij. of the aq. am. acetat. were substituted; nitre was added to his common drink in small quantity. Conditional orders were farther left with the hospital-mate, that if any pain of bowels should supervene, to apply hot fomentations without delay, and, failing of being able to procure any sufficient relief by the bowels, to give ʒij. of the Epsom salt every third hour with plentiful dilution; or if these means should prove insufficient, to give an ounce of castor oil, and throw up a purgative injection every two hours.

Die 4.—He had 4 stools since 9 o'clock last night, and without the clysters; still complains of pain in the head; much confusion and incoherency; face flushed; tongue not very foul; pain of belly removed; blister rose well on the abdomen, but not on the temples; they were accordingly ordered again to be put to them; an emollient poultice was directed likewise to the bubo, and the aq. am. acetat. to be discontinued, if the purging should go to excess.

Was ordered five grains of camphor three times a day.

Vespere.—Passed three stools since morning, great part of which consisted of scybala; vomited the camphor and aq. am. acetat.; respiration hurried; tongue not very foul; great headach; some pain of belly; the camphor was omitted, and some gum Arabic added to his common beverage. The aq. am. acetat. was thrown up, in consequence of which the saline mixture was given in its place.

Die 5.—Had a great many stools since the visit last night; some of them rather copious; appears relieved to day; pulse and respiration still more hurried; great anxiety in his looks; complains

now

now of no pain in his bowels; thirst great; slept a little in the night; the bubo causes much pain though not large. Was ordered five grains of camphor three times a day, and the muriatic acid as before. An emollient poultice was put to the bubo.

Vespere.—This evening is very restless and delirious; tears off his blisters; oppressed with constant nausea and retching; a blister was put to his forehead, repeating the saline mixture and camphor as before.

Die 6.—Very delirious in the night; tongue not very foul; will not drink his barley-water; expresses great distress from the pain of the bubo; affected with a degree of tenesmus. The muriatic acid was omitted.

Vespere.—Has had a great many trifling evacuations by stool. About 8 o'clock P. M. he died.

After death his lower extremities were all equally of a motley bluish cast, with some small petechiæ over his breast and arms. The bubo was completely stationary for some days previous to death. The tongue, though whitish, was remarkably clean throughout the whole of his illness.

II.

Observations on the Influence of Vaccination on other Diseases, and on Population in general. By ROBERT WATT, M. D. Lecturer on the Theory and on the Practice of Medicine, Glasgow.

To the Editors of the Edinburgh Medical and Surgical Journal.

GENTLEMEN,—I perceive by a critique in your last number, and by remarks from various other quarters, that my inquiry, respecting the fatality of measles, and the numbers who have died under ten years of age in Glasgow, is likely to produce a variety of discussion, and some difference of opinion. I shall be glad if it do; for these discussions always lead to improvement, provided they are conducted with that candour and temper which ought to characterize all philosophical investigations. But, unfortunately, it too often happens, that professional men run into opposite extremes. They either sink into a profound apathy, which renders them totally regardless of the interests of science; or their discussions are carried on in a strain of acrimony and invective, which no circumstances can either excuse or justify.

At

At first I resolved to take no share in any discussions to which the inquiry might lead, and I should have kept to that resolution, had it not appeared to me, that your reasoning frequently rested on what I considered to be very questionable grounds, and sometimes on mere hypotheses, when the facts, had you known them, would have led to very different conclusions. The truth must ultimately prevail, and I have no objection that it fall to that side of the question which you have espoused; but let it be ascertained by fair induction. The result of an investigation is often very different from what was expected; but whatever be the result, the inquiry itself, if conducted with candour and perseverance, will seldom fail to promote the interests of science.

You will perhaps say the facts are well enough; but the attempt to account for the result might have been spared. I question, however, if the tables, without the speculations, would have excited the same degree of interest which they have done. It is not because a fact is new, extraordinary, or unaccountable, that it excites attention; but because it is made to open new prospects to our view, to overturn old and established opinions, and, thus to extend the sphere of our knowledge. Nine-tenths of the discoveries in natural science have been made in attempts to establish or overturn particular theories.

You object to the length of time and the extent of field to which the tables apply. This I grant you is just, were I attempting to draw conclusions beyond the extent of my facts; but, as I have oftener than once observed in the inquiry, the conclusions apply merely to Glasgow. I leave it to the industry of others to produce similar tables to show how the matter stands in other places, and, till that is done, it is unnecessary to suppose local circumstances or any thing else. Facts can only be opposed by facts equally numerous and well ascertained.

You remark, that it does not correspond with your own observations, except in the year 1808, that measles have been more fatal of late than formerly, and to this you have added the firm belief of Dr Bevan, and you might also have subjoined the evidence in the possession of Dr B. of the Carey-Street Dispensary. But what does all this amount to? Had you asked myself a year ago, or I believe any other practitioner in Glasgow, the answer would have been the same. The result of the inquiry has not been less surprising to the practitioners of Glasgow than to those of other places. But why have recourse to hypotheses when direct evidence can be obtained? What do the London bills of mortality say? To ascertain this point, I take the last ten years, and compare them with the ten years previous to the commencement of vaccination. In the ten years from 1788

till 1797 inclusive, the deaths by measles were 1 in 75.7 of the whole deaths, and this is considerably above the average proportion of deaths by measles in the whole century. In the last ten years, viz. from 1803 till 1812 inclusive, the deaths by measles are as 1 in 31.7 of the whole deaths; or, to express it otherwise, the deaths by measles have increased in the last ten years, compared with the first ten years, as from 4 to 9, or they are considerably more than doubled. Taking the same two periods, you will find, that the deaths by small-pox, in the latter, are not reduced to a half of what they were in the former, but nearly as 9 to 5. In the one case then, we have the deaths reduced as from 9 to 5, and in the other, increased as from 4 to 9.

How does this ratio correspond with the deaths as given in the Glasgow tables, in the same periods? From 1788 to 1797 inclusive, the deaths by small-pox are as 1 in 5.1. From 1803 till 1812 inclusive, they are as 1 in 19.9. This then is a reduction, not from 9 to 5, as in London, but as from 20 to nearly 5. In what proportion have the deaths by measles increased? From 1788 till 1797 inclusive, the deaths by measles are as 1 in 65.7. From 1803 till 1812 inclusive, as 1 in 12.8. This then is an increase, not from 4 to 9 as in London, but from 4 to nearly 20.

Now I would ask, if any two statements could be expected to come nearer one another than these? Is not 20 to 5, and 4 to 20, pretty much the same as 9 to 5, and 4 to 9. Thus the London bills of mortality, in so far as the deaths by small-pox have been reduced in that city, go the same length as the Glasgow tables, in support of the inference I have drawn. And if the increase of deaths by the one disease has hitherto exceeded the decrease of deaths by the other, is it not fair to suppose, that, had the decrease gone farther, as it has done in Glasgow, the increase would have continued to bear the same proportion to it. This you will say is but on hypothesis. I grant you it is; but I think it a fair one, and one which, at all events, deserves to be investigated, and in a more satisfactory way than by quoting individual experience or observation, whatever may be the extent of the one, or the correctness of the other.

You doubt the connection between the decrease of the small-pox and the increase of measles. I am not disposed to enter into the metaphysical regions of causation, where men have been led to doubt even of their own existence; but I may remark, in general, that when we find one thing succeed in the same proportion as another is withdrawn, so as to produce the same general result, it is very natural to suppose they are somehow or other connected. I hold it to be equally as unphilosophical to be

be indiscriminately sceptical as to be indiscriminately credulous. But it often happens, that those who are most sceptical on the one side of a question, are the most credulous on the other. You are quite sceptical as to the result of an inquiry conducted on the most rational principles of induction, and with a degree of minuteness, I believe, hitherto unexampled; but you have no difficulty in yielding implicit faith to the circumstance of the Inverness Militia. This circumstance, you admit, was a mere probability. That probability, in the next instance, grows on your belief, and becomes a fact, and then a fact of so comprehensive a nature as not to leave the possibility of a doubt.

You have quoted a number of places to show, that the deaths in Glasgow under ten years of age have always been excessive. But I would beg leave to ask, Are you certain that the registers in all these places are kept as generally and as minutely as I have stated them to be kept in Glasgow? That much depends on this circumstance, will appear from the following fact. When I first began the investigation, I took in merely the three burying-grounds belonging to the city, and I found that the deaths among children under ten years of age were only about 46 per cent. of the whole deaths. When I came afterwards to take those of the suburbs separately, they exceeded 60 per cent. The consequence was, that, on throwing the whole into one, the average turned out to be 54.75 as you have stated. *

I have no means of knowing how the bills of mortality are made out, in most of the places you have quoted, and therefore I cannot speak as to their generality and correctness. With regard to London, they are made out in such a partial manner, that I should not be surprised to find as great a difference between the bills published, and the real state of mortality, as I found here between the registers of the city and those of the suburbs. In London, the bills only include those who are buried according to the rites of the church of England, and consequently they exclude the Jews, Quakers, Catholics, and the whole body of dissenters; and that these correspond, in some manner, with the poorer sort of the people, who are chiefly buried in the suburbs in Glasgow, is more than probable. The lowest

* It will be seen, however, by the subjoined bill of mortality for 1813, that the deaths under ten years of age, even in the city burying-grounds, now exceed 50 per cent. of the whole deaths. In this, measles do not appear to have had any very remarkable share. But in the inquiry it is not maintained that the deficiency occasioned by the decrease of small-pox is made up by measles alone. It is most distinctly stated, (in Inquiry p. 383,) that chincough, stopping, scarlet-fever, &c. may all have a share; and it appears from the present bill, that the two former of these have been very fatal indeed; and very probably a number of those registered under fever were cases of scarlatina.

lowest and most wretched of the inhabitants of London are to be found among the Jews and Catholics.

But

Mortality Bill of the city of Glasgow and suburbs, for the year 1813. Interred in the High Church, Black Friars, and North-West Church Yards.

	Males,	Fem.	Tot.
January,	47	47	94
February,	43	68	111
March,	36	6	92
April,	46	51	97
May,	49	54	103
June,	52	45	97
July,	39	48	87
August,	42	45	87
September,	40	46	86
October,	52	59	111
November,	69	62	131
December,	58	69	127
	573	600	1223

Whereof have died,

Age,			No.
Under 2 years,			284
Between 2	and	5,	151
5		10,	81
10		20,	55
20		30,	57
30		40,	78
40		50,	72
50		60,	68
60		70,	133
70		80,	94
80		90,	34
90		100,	3
			1223

Diseases.	No.
Abortive,	45
Accidental,	4
Aged,	256
Apoplexy,	3
Asthma,	23
Bowelhive,	118
Burnt,	3
Childbed,	18
Chincough,	120
Cholic,	1
Consumption,	229
Dropsy,	10
Drowned,	3
Fever,	99
Gravel,	1
Inflammation,	28

Diseases.	No.
Jaundice,	1
Killed,	2
Measles,	55
Mortification,	1
Palsy,	4
Rupture,	1
Small-pox,	42
Sore throat,	5
Stopping,	88
Suddenly,	13
Swelling,	4
Teething,	20
Water in the chest,	3
Water in the head,	28
	1223

Interred in the High Church Yard,	645	738
In do. from the Infirmary,	83	
In Black Friars and North West Burying Grounds,		578
In Episcopal Chapel, do. do.		24
In Town Hospital, do. women, 40		81
men 25, children 16		
Total in the City,		1421
In Calton Burying Ground,		446
In Bridgeton, do.		107
In Gorbals, do.		420
In Anderston, do. Relief,	212	310
In do. do. Cheapside,	98	
Total in the City and Suburbs,		2704
Total in the year 1812,		2716
Decrease this year,		12

But this is not the only partiality; there are other circumstances, which may have a share in altering the proportion of deaths under ten years of age. Besides the many burying-grounds belonging to the different religious denominations, several very extensive public cemeteries are entirely omitted. Of these, I may mention St Paul's cathedral, Westminster Abbey, the Temple, the Rolls, Lincoln's Inn, St Peter's in the tower, the Charterhouse, the numerous hospitals, and even the great parishes of Marybone and Pancras; in the former of these alone, the average number of burials exceeds 1500 per annum *. At the time Mr Pennant published his account of London, in 1791, it was believed that there were nearly as many burials without, as within the bills of mortality, and if this was the case then, it is likely to be still more so now.

You remark, that it appears from the tables, that the number of deaths by small-pox, in Glasgow, prior to vaccination, was also excessive; and in support of this opinion, you have quoted a number of authorities. As to the first of these, viz. London, you can easily see how liable it is to deception, from its not comprehending the whole deaths of the city; and it remains to be shewn, whether the same objection does not apply to Geneva, Edinburgh, Berlin, Leipzic, Vienna, Breslau, and Lebin; as to Salzwedel, and the 140 German villages, they come so near that of Glasgow, as to do away, in a great measure, the idea of its being in this respect singular. Indeed, that of the 140 German villages appears to be the best authority you have produced; for this reason, that it is not only an extensive average, but, in each of these villages, there would very probably be only one burying-place, so that there was no room left for deception. I may also add, that the proportion of deaths by measles, in the 140 German villages, stands much higher than even in Glasgow, and nearly double what it does in the other places you have quoted. I would be disposed to ascribe this to the same cause, a more complete registration.

You remark, in opposition to the conclusions drawn from the Glasgow tables, that, in other places, vaccination has had a considerable effect in diminishing the deaths under ten years of age, and you strengthen your opinion by the authority of Dr Heberden; but as Dr Heberden's own authority is merely that of the London bills of mortality, I have already shewn how liable it is to error. By confining my first researches to the city registers,

* Heberden's Observations on the Increase and Decrease of Certain Diseases, p. 6.

I obtained a saving of from five to six per cent. and especially in the last eight or ten years; but when I came to extend them to the whole city and suburbs, in place of a saving, I discovered a loss. Before any general conclusion is drawn, let the same thing be tried with regard to London.

But there is one of Dr Heberden's results, which corresponds exactly with that of Glasgow; namely, a considerable saving of lives under two, but a great increase of deaths between two and ten. In Glasgow, the increase of deaths, between two and ten, is nearly six per cent. of the whole deaths in the city and suburbs. In fact, it appears from the tables, that a child never had a better chance of reaching its tenth year, than between 1783 and 1789 inclusive, when nearly a fifth part of the whole deaths in the city and suburbs were occasioned by small-pox.

Let us next examine the proofs from France. These may be comprehended under four heads: 1st, Those which shew a diminution in the aggregate of deaths. 2d, Those which show that the aggregate of population is increased. 3d, Those which show that there is a progressive increase of births, compared with the number of deaths. And, lastly, the direct proofs which shew that there is a real diminution of deaths under ten years of age. I shall notice each of these separately.

I. As to the first, it may be remarked, that a variety of causes may operate in diminishing the number of deaths in a particular place, and with regard to France, some of these causes have of late years been abundantly obvious. How many thousand Frenchmen have paid the debt of nature, without adding to the list of deaths in their native cities? But laying little or no stress on this circumstance, out of the immense population of France, you have given only a very few towns, some of them indeed mere villages, and even in these the calculations apply only to a few years. In none of them is it nearly equal to the period given in Glasgow.

But fatal as disease appears to have been in Glasgow, the following table will shew, that the deaths now bear a much smaller proportion to the aggregate of population, than they did formerly. The population of Glasgow was ascertained in 1780, in 1785, in 1791, in 1801, and in 1811. These 32 years I divide into four unequal periods. The I. consisting of six years from 1780 till 1785, inclusive. The II. consisting of six years, from 1785 till 1791. The III. consisting of ten years, from 1791, till 1801; and the IV., consisting also of ten years, from 1801 till 1811. In each period, taking the average population, and the annual average of deaths, the proportions stand thus:

Periods.

Periods.	Population.	Deaths.	Proportion.
I.	44,360	1661	1 in 26.7
II.	56,233	2012	1 27.9
III.	75,173	2127	1 35.2
IV.	96,997	2377	1 40.8

If the number of deaths under ten years of age had not been ascertained by the tables, this extraordinary disproportion between the deaths and the population, would have been chiefly ascribed to the effects of vaccination; and as vaccination has prevailed more in Glasgow than almost anywhere else in the kingdom, it would have been held up as a triumphant proof of its influence on population in general. But has it had no effect? I am far from either thinking or saying so. Though the rapid increase of population in Glasgow must be partly ascribed to the influx of people from other places; yet I have no doubt that it has also proceeded from the number of births far exceeding the number of deaths; and, that vaccination has snatched many names from the list of deaths, and added them to the aggregate of population, is I think more than probable. It has been proved, that as great a proportion die under ten years of age as ever, but this does not necessarily imply, that there is not also a greater number living at that age than in any former period.

2d, As to the increase of population, I do not see that any inference can be drawn, either for or against the question. The increase of population in Glasgow, where the deaths appear to have been so excessive, will bear a comparison with that of any other city in Europe. In the year 1780, the city and suburbs contained 42,832 inhabitants; in 1785, they contained 45,889; in 1791, 66,578; in 1801, 83,769; and, in 1811, the last survey that has been made, they contained 110,225. To this I shall add a small table, taken from a late history of Glasgow, to show, that, in point of healthiness, Glasgow is not inferior to other places. I do not vouch for its accuracy: it is given merely as I found it.

"The proportion of inhabitants which die annually in cities and large towns, on an average of three eras in the preceding twenty years:

In Vienna, . . .	1 in 19½	In London, . . .	1 in 20½
Berlin, ...	1 20½	Liverpool, . .	1 27$\frac{7}{10}$
Manchester, .	1 28	Edinburgh, .	1 30
Glasgow, . .	1 39$\frac{8}{12}$	Paris, ...	1 21."*

3d,

* Chapman's Picture of Glasgow, published 1812, p. 244.

3*d*, With regard to the increase of births in France, the two examples you give afford but a very slight evidence. Besides being local, they apply merely, in the one case to four, and in the other to five years. The time is, therefore, too limited to admit of any general inference; and, moreover, we are not told whether there may not have been a similar increase prior to 1806, in the one case, and 1807 in the other. On this head I have no evidence to offer. The births in Glasgow are registered in a very imperfect manner. This arises partly from the negligence of parents, and partly from the great number of dissenters. The sum levied by the keeper of the registers is too small to make it an object for him to look after the defaulters. It is much to be regretted, that some more effectual measures are not adopted by the legislature to insure an accurate registration of births. There is no reason, however, to doubt, from the rapid increase of the population of Glasgow within these last 12 years, that the number of births has greatly exceeded the number of deaths. The operatives here marry very young, and though their progeny may not be the most robust, it is far from being deficient in point of number. Your remark with regard to the increase and enlargement of schools, seems to be still less to the point. You might as well ascribe the late furor about Lancasterian schools in this kingdom to the same cause. Of this kind of evidence of increased population we have three or four splendid examples in Glasgow.

4*th*, Lastly, as to the direct proofs of a diminution of deaths under ten years of age, the examples afforded are not very numerous. In the reduction of deaths at Bensancon, the reporter says, that it was chiefly among those under ten years, but the remark applies merely to a period of nine years, and a district where not a thousand died annually. In Toulon there seems to have been a considerable reduction of deaths under fifteen years, but this applies merely to a period of five years. Take the first five years in Glasgow after vaccination was introduced, say from 1802 till 1807 inclusive, and you will find a similar, if not a greater reduction. The only other proof that remains to be noticed, is that of M. Petiet, physician at Gray. He states, generally, that, since vaccination has been introduced into that circle, that the deaths under five years have annually been *sensibly* lessened. So it also happens in the 4th period of the Glasgow Tables: but what was the final result?

Thus, gentlemen, have I carefully and dispassionately examined the proofs you have collected to invalidate the conclusions I have drawn respecting the effects of vaccination on other diseases, and on population in general. I feel indebted to you for

for the pains you have taken, and I give you credit for the zeal you have shewn; but I trust you will also receive the observations I have thought it my duty to make, in the same spirit of candour in which they were written. Nothing would have induced me to embark in support of an opinion so contrary to established notions, but a conviction of its importance, and a desire to discover the truth. If you, or any of your readers, from the side I have taken, should think me hostile to the cow-pox, I can assure you that you are very much mistaken. Indeed that inestimable discovery has suffered much more from the forwardness of its friends, than the strength of its enemies. I have been led into my present belief not designedly, but by the discovery of what I conceive to be the most unquestionable facts, and nothing can alter that belief, but facts equally general and well ascertained.—I am, &c. ROBERT WATT, M. D.

Glasgow, 18th January 1814.

P. S. I am much pleased with the following very pertinent remark you have made in your review of Dr Warren's cases of diabetes, and therefore beg leave to direct the attention of your readers to it. "It has been observed by some practitioners, that the removal of the urinary disease, by animal diet, opiates, &c. is by no means always equivalent to a cure of the constitutional malady, and that the emaciation, the *tabes*, or consumption connected with it, still goes on to its fatal termination."* This accords completely with my observation, and I must say, that I can hardly conceive any thing more frivolous or puerile, than to see men of common sense pleasing themselves, and building their hopes on drams and ounces of urine, while death from every other quarter is staring them broad in the face. I have elsewhere stated†, that we have no direct proof of any one of Dr Rollo's patients having completely recovered, and afterwards enjoyed a course of good health. On the contrary, the conclusion of the history of their recovery is generally an account of their death, or they pass to a distance, or something else intervenes to leave the mind in a state of doubt and uncertainty. The same remark applies to the cases of Dr Bardsley, and in fact to almost all, if not the whole cases which have been published since. That complete cures, in confirmed cases, are not very often accomplished, the more my experience extends, the more I am ready to admit; and the reason is, that we seldom have confirmed

* See p. 113 of the last number of this Journal.

† Edinburgh Medical and Surgical Journal, Vol. V. p. 297.

firmed cases without their being combined with some other fatal affection. I can say, however, what I believe few other authors on the subject can say, that all the cases I have published as cured remain so, and are alive at this day. Stevenson is a stout healthy ploughman in this neighbourhood. Mr J. C. has completed his studies, and is now an established clergyman; and Mrs Caldwell is a stout healthy woman of 63 years of age.

With regard to the success of my practice, since the publication of the cases in 1808, it is difficult to form an accurate estimate. In that time, I have had a number of cases under my care, and I have been consulted respecting many more. The general impression on my mind is, that the practice will succeed where any plan of treatment is likely to be of service. The obstinacy of some cases, and the peculiarity of others, have led to many modifications, and to a variety of results. I have also had favourable, and less favourable accounts from almost every quarter of the kingdom; and I can say, from all those, that the practice, even where it seems to have done little or no good, never did harm; and I have never even heard of one of these sudden *exits*, which are so frequently the consequence of *cures* by other means. As I intend, at some future period, to give the result of my more mature experience to the public, I shall not at present take up your time on that subject, except merely to say, that, for some years, I have found repeated emetics a very useful addition to the other parts of the treatment I formerly recommended. R. W.

III.

Case of Aneurism from a Wound, in which the left Carotid Artery was tied. By ALEXANDER MACAULAY, late surgeon of the Coldstream, now at Leith.

IN Calcutta, December 11th, 1812, at half-past eleven P. M. Mr WILLIAM JONES, gunner of the Honourable Company's ship Coldstream, aged 36, fell from a chest upon some glass ware, and received two wounds, one about the middle of the masseter muscle of the left side, which was slight; and the other, about an inch long, and three-eighths of an inch deep, in a longitudinal direction, between the mastoid process and the articulation of the

the jaw on the same side. In this last, an artery of considerable size was wounded, and bled profusely. Stopped the bleeding by manual compression of the carotid artery. Next morning, brought the edges of the wound together by suture, and both wounds appeared to do well; till, on the 28th of December, seventeen days after the accident, hæmorrhagy returned three different times, at one P. M. at six P. M. and at one next morning. It was always stopped by compression. It again took place on the evening of the 31st, when much stronger compression was applied, about an inch below the wound, and the bleeding stopped for the present.

January 5th, 1813.—Removed the compress to-day, and found a small aneurismal tumour close to the edge of the wound. Renewed the compression.

7th.—Examined the wound, and found nearly the same appearances. Applied simple dressing to the wound, which is now nearly healed. Discontinued the compression.

9th.—The aneurismal tumour does not increase in size.

Contin. ung. simplex.

10th.—The tumour to-day appeared very near bursting. Attempted to take up the artery a little below the original wound, but did not succeed. Punctured the tumour, from which florid blood immediately issued *per saltum*; made a small incision in continuation of the wound, and stopped the bleeding by ligature.

14th.—Continues to do well; but at times feels pain in the neighbourhood of the wounds; sometimes in the internal ear. Bowels costive.

Sumat calom. et jalap. āā gr. x.

16th.—In the course of the two last nights, he has lost a quantity of blood from his wounds. To-day, the aneurismal tumour is larger than it has yet been. Enlarged the original wound, and endeavoured to discover the bleeding vessel, but could not. As he was losing blood very fast, and as I suspected I had wounded the carotid artery, near the foramen carotideum, I thought it advisable to take it up at the lower part of the neck; which I did by making two ligatures, and cutting between them. As I was doubtful of the one next the heart, I put one more on that part of the vessel, previous to dividing it. The ligatures between which I cut were about an inch distant. Two hours after the operation, pulse 96. In the evening, skin hot and dry; little thirst; pulse 120, and sharp.

17th.—Complains of headach, but not particularly of the left side; rather across the forehead. Pulse more moderate.

18th.—Removed the dressings; wound looks well. Pulse last.

last night 120; skin then hot and dry. Pulse this morning 108; thirst moderate; some headach, but not severe or constant. One stool. Takes chicken-broth and other liquids without much pain or difficulty.

19th.—Smart febrile symptoms last night; much thirst, heat, and dryness of skin; great restlessness. Gave ℥iss. spir. mind. in ℥iv. of water. About one A. M. the severity of the symptoms abated, the pulse became quite moderate; and a gentle perspiration came out over the whole body. This morning he has great drowsiness, but no delirium. Pulse 84, and feeble; surface disposed to be cold.

20th.—Pretty good sleep last night; much languor and drowsiness; little thirst; appetite good; pulse 96; the right side of the body is much more feeble than the left.

21st.—Slight delirium last night; numbness and feebleness of the right arm is less; but that of the inferior extremity continues; appetite continues good; one copious stool to-day.

Utatur vino rubro sæpius in die. To have strong beer.

22d.—Strength beginning to return; much discharge of good pus from the wound; pulse 90; right side recovering.

Sumat haust. anod. vespere.

23d.—Some headach and sickness, probably from the opiate; in other respects doing well.

25th.—Complains of pain about the temporal muscle when eating; ligatures still very firm; has complained of pain, apparently in the course of the subclavian artery, but it is now diminishing; pulse 84.

Sumat quotidie, cinchon. ʒi. vini rubri ℥ii.

26th.—Pulse 84; appetite good; bowels rather slow; much debility; no headach.

Contin. cinchon. et vinum.

28th.—Complains of much pain for some distance round the zygoma, during eating or swallowing. The ligatures are firmly attached, and keep up a considerable discharge.

Omit. cinchon. Contin. vinum.

29th.—Pain less to-day; wound looking well.

31st.—Little pain; discharge still copious.

App. cerat. vulneri.

February 3d.—The ligatures on the portion of the artery next the heart came away with but little force; but he complained for a few minutes of much pain in the chest; great pain is felt on any attempt to move the other ligature.

6th.—The last ligature came away, but with considerable pain of the neighbouring parts; slight hardness, and redness around the

the original wounds; thus the first ligatures remained 18 days, and the other 21.

19th.—The original wounds are healed up, but a slight degree of pain is still felt when pressure is made in the neighbourhood of them; the wound from the operation did not close accurately; and a fungous growth rises between its sides, requiring to be kept down by caustic; general health good; strength returning.

22d.—Occasionally has a pricking sensation about the wounds; still some fungous growth at the place of the operation; general health good.

Returned to duty.

The fungus, about the size of a pea, continued nearly three months. The man returned to England in August, in perfect health.

IV.

Remarkable Instance of Irregularity in the Formation of the Urinary and Generative Organs. By J. G. Smith, M. D. Assistant-Surgeon 12th Light Dragoons.

In the month of May, 1812, I took charge of the hospital of the cavalry depôt, at Dorchester, in the county of Dorset. Among other patients, I found a man belonging to the ninth light dragoons, convalescent from diarrhœa, or mild dysentery. He had so well recovered, that I had fixed the day for his discharge to duty. On the morning of that day, however, I found him complaining of headach, and, on examination, discovered that he was affected with alvine constipation. Conceiving that his new malady might arise from this irregularity of the abdominal functions, I directed my practice accordingly, but without success. The primæ viæ were with difficulty evacuated, and that but imperfectly, after the repeated administration of cathartics.

This complaint occurred on the 29th May. On the 31st, I found the patient delirious; dilated pupils; restlessness; frequent moaning; and outrageous conduct, when disturbed, were the only visible symptoms. The torpor of the alimentary canal still continued. Shaving and blistering the head availed nothing. On the 2d of June, I found his face flushed, and unequivocal marks of determination towards the head, for the first time. He

was

was bled, and this practice was followed up by repeated doses of submurias hydrargyri, with other purgatives, and enemata. Copious dejections per anum were now produced; but he died on the day following.

The patient was 23 years of age, of a spare habit of body, with a proportionate head, and long neck. There was no mark of plethora; on the contrary, his natural make had been further fortified against such a diathesis by the assistance of disease.

On opening the body, great distention of the cerebral vessels was discovered; and the lateral ventricles were filled with serous fluid.

In the thorax there existed great pulmonary disease; strong adhesion of the pleura; the right lung gorged with purulent matter, and the left one tuberculous throughout.

There were no morbid appearances in the abdomen; but I discovered the following anomalous structure:—

In the left side, no kidney, no ureter, no spermatic cord; in the situation of the testicle a small soft degenerated body. On the right side the kidney and ureter were healthy, and about one-fourth part larger than they are usually found; the testicle, smaller than common, was in the pelvis.

Mr Edwards of Dorchester, a most respectable practitioner, was kind enough, at my request, to call and satisfy himself that every thing was as I have above detailed.

Portugal, 14th January 1814.

V.

On the Fracture of the Carpal extremity of the Radius. By A. Colles, M. D. one of the Professors of Anatomy and Surgery in the Royal College of Surgeons in Ireland.

THE injury to which I wish to direct the attention of surgeons, has not, as far as I know, been described by any author; indeed the form of the carpal extremity of the radius would rather incline us to question its being liable to fracture. The absence of crepitus, and of the other common symptoms of fracture, together with the swelling which instantly arises in this, as in other injuries of the wrist, render the difficulty of ascertaining the real nature of the case very considerable.

This

This fracture takes place at about an inch and a half above the carpal extremity of the radius, and exhibits the following appearances.

The posterior surface of the limb presents a considerable deformity; for a depression is seen in the fore-arm, about an inch and a half above the end of this bone, while a considerable swelling occupies the wrist and metacarpus. Indeed, the carpus and base of metacarpus appear to be thrown backward so much, as on first view to excite a suspicion that the carpus has been dislocated forward.

On viewing the anterior surface of the limb, we observe a considerable fulness, as if caused by the flexor tendons being thrown forwards. This fulness extends upwards to about one-third of the length of the fore-arm, and terminates below at the upper edge of the annular ligament of the wrist. The extremity of the ulna is seen projecting towards the palm and inner edge of the limb; the degree, however, in which this projection takes place, is different in different instances.

If the surgeon proceed to investigate the nature of this injury, he will find that the end of the ulna admits of being readily moved backwards and forwards.

On the posterior surface, he will discover by the touch that the swelling on the wrist, and metacarpus, is not caused entirely by an effusion among the softer parts; he will perceive that the ends of the metacarpal, and second row of carpal bones, form no small part of it. This, strengthening the suspicion which the first view of the case had excited, leads him to examine, in a more particular manner, the anterior part of the joint; but the want of that solid resistance, which a dislocation of the carpus forward must occassion, forces him to abandon this notion, and leaves him in a state of perplexing uncertainty as to the real nature of the injury. He will, therefore, endeavour to gain some information, by examining the bones of the fore-arm. The facility with which, (as was before noticed,) the ulna can be moved backward and forward, does not furnish him with any useful hint. When he moves his fingers along the anterior surface of the radius, he finds it more full and prominent than is natural; a similar examination of the posterior surface of this bone, induces him to think that a depression is felt about an inch and half above its carpal extremity. He now expects to find satisfactory proofs of a fracture of the radius at this spot. For this purpose, he attempts to move the broken pieces of the bone in opposite directions; but, although the patient is by this examination subjected to considerable pain, yet, neither crepitus nor a yielding of the bone at the seat of fracture, nor any other positive evi-

dence

dence of the existence of such an injury is thereby obtained. The patient complains of severe pain as often as an attempt is made to give to the limb the motions of pronation and supination.

If the surgeon lock his hand in that of the patient's, and make extension, even with a moderate force, he restores the limb to its natural form, but the distortion of the limb instantly returns on the extension being removed. Should the facility with which a moderate extension restores the limb to its form, induce the practitioner to treat this as a case of sprain, he will find, after a lapse of time sufficient for the removal of similar swellings, the deformity undiminished. Or, should he mistake the case for a dislocation of the wrist, and attempt to retain the parts *in situ* by tight bandages and splints, the pain caused by the pressure on the back of the wrist will force him to unbind them in a few hours; and if they be applied more loosely, he will find, at the expiration of a few weeks, that the deformity still exists in its fullest extent, and that it is now no longer to be removed by making extension of the limb. By such mistakes the patient is doomed to endure for many months considerable lameness and stiffness of the limb, accompanied by severe pains on attempting to bend the hand and fingers. One consolation only remains, that the limb will at some remote period again enjoy perfect freedom in all its motions, and be completely exempt from pain: the deformity, however, will remain undiminished through life.

The unfavourable result of some of the first cases of this description which came under my care, forced me to investigate with peculiar anxiety the nature of the injury. But while the absence of crepitus and of the other usual symptoms of fracture rendered the diagnosis extremely difficult; a recollection of the superior strength and thickness of this part of the radius, joined to the mobility of its articulation with the carpus and ulna, rather inclined me to question the possibility of a fracture taking place at this part of the bone. At last, after many unsuccessful trials, I hit upon the following simple method of examination, by which I was enabled to ascertain, that the symptoms above enumerated actually arose from a fracture, seated about an inch and a half above the carpal extremity of the radius.

Let the surgeon apply the fingers of one hand to the seat of the suspected fracture, and, locking the other hand in that of the patient, make a moderate extension, until he observes the limb restored to its natural form. As soon as this is effected, let him move the patient's hand backward and forward; and he will, at every such attempt, be sensible of a yielding of the factured ends

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of the bone, and this to such a degree as must remove all doubt from his mind.

The nature of this injury once ascertained, it will be a very easy matter to explain the different phenomena attendant on it, and to point out a method of treatment which will prove completely successful. The hard swelling which appears on the back of the hand, is caused by the carpal surface of the radius being directed slightly backwards instead of looking directly downwards. The carpus and metacarpus retaining their connections with this bone, must follow it in its derangements, and cause the convexity above alluded to. This change of direction in the articulating surface of the radius is caused by the tendons of the extensor muscles of the thumb, which pass along the posterior surface of the radius in sheaths firmly connected with the inferior extremity of this bone. The broken extremity of the radius being thus drawn backwards, causes the ulna to appear prominent toward the palmar surface, while it is possibly thrown more towards the inner or ulnar side of the limb, by the upper end of the fagment of the radius pressing against it in that direction. The separation of these two bones from each other is facilitated by a previous rupture of their capsular ligament; an event which may readily be occasioned by the violence of the injury. An effusion into the sheaths of the flexor tendons will account for that swelling which occupies the limb anteriorly.

It is obvious that, in the treatment of this fracture, our attention should be principally directed to guard against the carpal end of the radius being drawn backwards. For this purpose, while assistants hold the limb in a middle state between pronation and supination, let a thick and firm compress be applied transversely on the anterior surface of the limb, at the seat of fracture, taking care that it shall not press on the ulna; let this be bound on firmly with a roller, and then let a tin splint, formed to the shape of the arm, be applied to both its anterior and posterior surfaces. In cases where the end of the ulna has appeared much displaced, I have laid a very narrow wooden splint along the naked side of this bone. This latter splint, I now think, should be used in every instance, as, by pressing the extremity of the ulna against the side of the radius, it will tend to oppose the displacement of the fractured end of this bone. It is scarcely necessary to observe, that the two principal splints should be much more narrow at the wrist than those in general use, and should also extend to the roots of the fingers, speading out so as to give a firm support to the hand. The cases treated on this plan have all recovered without the smallest defect or deformity of the limb, in the ordinary time for the cure of fractures.

I cannot conclude these observations without remarking, that were my opinion to be drawn from those cases only which have occurred to me, I should consider this as by far the most common injury to which the wrist or carpal extremities of the radius and ulna are exposed. During the last three years, I have not met with a single instance of Dessault's dislocation of the inferior end of the radius, while I have had opportunities of seeing a vast number of the fracture of the lower end of this bone.

Stephens Green, February 21, 1814.

VI.

On the Use of Venesection in Diabetes Mellitus. By Joseph Ayre, M. D. Physician to the Hull General Infirmary, and to the Penitentiary, and to the Lying-in Charity of Hull, &c. &c.

Among the many valuable communications made to your Journal, it appears somewhat extraordinary, that so little has been written concerning the employment of venesection, as recently recommended by Dr Watt, in Diabetes Mellitus. The importance of the remedy, as well as the hitherto intractable if not incurable nature of the disease, not to mention the respectable authority by which the practice comes recommended, appears to entitle it to as full a share of our regard, as any at least of the crowd of new medicines that are daily presented to us. The practice, it is true, of ascribing efficacy to new medicines upon insufficient data, too common at all periods of the history of medicine, and much too frequent in this, may be expected to have excited in many a distrust in new remedies, and an undue reliance upon old ones; and may probably have militated still more strongly against the practice in question, from the additional consideration that its effects are by no means inert, nor very consonant with our preconceived notion of the curative indications. The very marked success, however, which was declared to have resulted from venesection, in the cases detailed by Dr Watt, and the almost utter uselessness of every other means, justified the hope of its receiving a very general trial; and yet the silence, with one or two exceptions, observed upon the subject, suggests the conclusion, that either the treatment has not been adopted, or its results have been unfavourable. In the following case of the

the disease, a very liberal trial will be found to have been given to it, and a hope, by no means ill-grounded, was long entertained of complete success; but though doomed to encounter disappointment on this head, yet the very powerful impression which venesection produced upon the disease, has encouraged the belief that a detail of the symptoms, with the effects of the remedy, and the circumstances under which it was used, may afford further views of this novel treatment, and lead to a farther, and, it is to be hoped, a more successful trial of it.

Mr C. of Castlethorpe, near Brigg, in Lincolnshire, farmer, aged 34, of laborious and temperate habits, and of robust form, was first seized with symptoms of diabetes mellitus, on the third day of a journey, in which he had undergone considerable bodily fatigue, and been exposed to much cold and rain, having taken, during that time, but a very few hours of natural repose. About the middle of April 1811, and six weeks after the attack, he applied to Mr Rudkin, a respectable surgeon of Brigg, and shortly after a physician of this place was consulted. From the commencement of the disease, the thirst had rapidly become urgent, and at this period was literally unquenchable, demanding, as ascertained by several trials, from 35 to 40 English pints of water, as the smallest quantity that could be dispensed with in the 24 hours. The urine was pale, and very sweet, corresponding, as it was conjectured, in quantity with the fluids taken, and voided almost involuntarily. The pulse small and quick; tongue white; skin very dry; appetite voracious; and bowels habitually and obstinately costive.

Immediately after consulting these gentlemen, he was put upon an animal diet, and directed to avoid all vegetable matters, and to take lime water for his common drink. During the space of nine months, he strictly adhered to this regimen, and took the following medicines with the utmost regularity, viz. Peruvian bark, rhatany root, extract of logwood, alum, gum kino, sulphate of copper, tincture of cantharides, opium combined with sudorifics, the volatile and vegetable alkalies, the sulphuret of potass, and hepatized ammonia, but with only occasional and very trifling benefit. Towards the close of December, I was consulted, and found him labouring under the disease in its most aggravated form. His thirst was excessive, and such was its intensity towards evening, that his frequent calls for drink and micturition precluded all repose, until many hours after midnight. He was considerably reduced in flesh, and incapable of walking but a very short distance. His urine was highly saccharine, and in other respects indeed so altered, that it was his

common

common practice to prefer it for rinsing his mouth, as the most effectual means he possessed for removing its clamminess. The voraciousness of the appetite, and the unperspirable state of the skin were unabated, and the bowels were still very costive. Sensible that a very full trial had been given to the ordinary remedies for this disease, and under the full assurance that nothing but death could result from their continuance, I determined to adopt a different plan, and having explained my views to the patient, and obtained his consent to the measure, I directed him to lose ℥xviii. of blood. This quantity, for reasons that need not be explained, was taken in two bleedings, viz. on the 6th and 10th of January, by Mr Rudkin, with whom I corresponded, and to whom I am much indebted for the following reports, as well as for the obliging and intelligent manner with which he seconded my wishes.

1812, January 6th—13th.—After the first bleeding, [the patient's pulse was increased from 80 to 96 in a minute, and is now 100, and weak. The symptoms of the disease are considerably abated; thirst not so excessive; the tongue, which has been white from the commencement of the complaint, is now but little so; urine less saccharine, particularly of a morning. The blood was of a dark colour, coagulating slowly, and the serum thick and adhesive, and of a milky appearance, but most so at the first bleeding. Lime-water and animal diet continued, with an allowance of porter at the patient's request. The bowels kept regular by means of the oleum ricini.

13th—18th.—From the favourable change produced by the first bleeding, 16 ounces were taken yesterday. The blood was strongly marked with the buffy coat, but the pulse did not rise. The improvement, however, upon the whole is considerable.

18th—23d.—Since the last report, the thirst had increased, and the urine had become more abundant and saccharine; 16 ounces of blood were taken yesterday. The buffiness less considerable than at former bleedings, but the good effects are more considerable. Mr C. feels his spirits and strength much improved, and thinks himself considerably better after this bleeding than after any of the former; the pulse is also stronger and less frequent, and the tongue clean.

23d—30th.—From the very considerable benefit derived from venesection, 20 ounces were taken on the 26th, which exhibited no buffiness on the surface. In the evening, feeling himself languid, he ate very heartily of fat meat, which appeared to disorder his stomach, and on the following day he complained of dizziness, and increased thirst. Urine sweet; pulse 110, and feeble; and tongue

tongue white. A bolus of calomel was given, which afforded relief, and the symptoms have gradually abated in violence.

January 30th—February 7th.—Has considerably improved since the last report. Yesterday 18 ounces of blood were taken, which was florid and free from siziness, though the serum was still milky; pulse 90, full and stronger; has gained nine pounds in weight since he was first bled.

7th—13th.—For several days past has pursued his farming work; 16 ounces were taken yesterday, which was without buffiness, but the serum was still milky. Feels his spirits and strength greatly improved; sleeps well; thirst quite moderate, though the tongue is white, and the skin continues dry; urine occasionally saccharine, though not immoderate in quantity, having seldom occasion to rise more than once in the night.

13th—21st.—From an observation made by Dr Watt, that an animal diet retarded the cure, a portion of vegetable food was begun with on the 16th. In the evening his thirst was increased, and the urine became sweet, with much prostration of strength. The night was restless, and the appetite was again voracious. These symptoms continued to increase with such violence, as to render a return to an animal diet indispensable: 18 ounces of blood were also taken, which was very sizy, but which had the immediate effect of removing the urgent symptoms. At one time, during the three days in which he used the vegetable diet, such was the intensity of his thirst, that he drank a gallon of water in 20 minutes.

21st—30th.—Since last report, nearly all the symptoms of diabetes have disappeared. The thirst and appetite are quite natural; urine in all respects natural; sleep quiet; pulse 86, and of good strength; spirits excellent; skin moist.

February 30th—March 27th.—Since last report there has been no return of the diabetic symptoms, and the general health and strength are so much improved, that Mr C. goes about the ordinary business of his farm. After some laborious exertions on the 16th, he had a little increase of thirst; and, at his own request, 16 ounces of blood were taken on that day. Has gained seventeen pounds in weight since he was first bled.

March 27th—June 15th.—Since last report has had occasional returns of thirst, for which he was bled on the 2d of April, to the amount of 16 ounces, with instant relief, and 12 ounces were taken on the 9th, and the same quantity on the 30th of May, with the same result. Since that time, he has continued perfectly well, engaging in the most laborious employments of his farm, and rising at three or four every morning.

After the lapse of many months from the date of the last report,

port, I had two or three times the satisfaction of seeing and learning from my patient, that he had continued perfectly well, and had acquired farther increase of weight. In the spring of this year however, and about twelve months after his recovery, he came over to Hull for my advice, when I learnt that he had been labouring under a return of his disease for a few weeks. His complaint was now in many respects different from the former state of it. The common symptoms of diabetes mellitus were much less urgent, but the debility and languor were greater, and, what was still worse, there was a tightness in the chest, attended by dyspnœa and cough. Medicines were ordered for the pectoral symptoms, and 16 ounces of blood were taken, which afforded some little relief. Shortly after this, he was attacked by a disease of an erysipelatous nature, at that time prevailing, in which small blisters arose in different parts of his body, attended by fever, and followed by troublesome sores. I now saw him again, and found his pectoral symptoms much the same, but his debility much greater, and evidently increased by venesection, which had again been tried. It was at this time I learnt from him, with not less surprise than regret, that he had been in the practice, for many months past, of eating a portion of flour-pudding daily, and of making an unsparing use of potatoes. A strict adherence to an animal diet was recommended, but, under the very unfavourable circumstances then existing, it gave but little promise of benefit. In fact, the debility and other symptoms increased. The bold but successful practice employed in his former attack was now no longer admissible. The fatal crisis appeared fast approaching. During the succeeding fortnight he became gradually worse, and, towards the close of it, his dyspnœa was suddenly and alarmingly increased, so as to demand an attempt at relief by means of venesection. This however failed, and he shortly after became comatose, and in a few hours expired.

Before proceeding to make any remarks upon the foregoing case, it is proper to observe, that wine and porter were allowed the patient, which he drank freely, but always temperately of; and that, besides venesection, a few medicines were given, though chiefly with the view of keeping the bowels open, and procuring a perspirable state of the skin. The former of these objects was at all times obtained with difficulty, and the latter one was never accomplished until the disease itself was removed, and then it seemed rather the concomitant than the cause of health.

That many of the facts in the above case are singular, and not unworthy of consideration, will be readily admitted. Cold and fatigue

fatigue were evidently the exciting causes of the disease. The appearances of the blood corresponded very nearly with what was observed by Dr Watt; and, indeed, so close was the relation betwixt the appearance of the blood and the relief afforded, that the amount of the one was pretty nearly determinable by a view of the other. During the first month, he lost 88 ounces of blood, and took at least only half the usual food, yet he gained nine pounds in weight, and acquired sufficient strength to follow a laborious employment. The pectoral complaint, which formed the principal part of his disease in the second attack, had certainly no place in the first one. It seemed rather to supervene upon the diabetes mellitus than to form a part of it, though it must, at the same time, be admitted, that it forms no uncommon termination of that disease. The very extraordinary success that followed venesection, in the first attack, would lead to the conclusion, that the same remedy, under a similar diabetic regimen, would have led to the same result. The important fact, however, so long concealed, of his departure from that regimen, became known too late to afford a fair opportunity for the trial; and when we add to the debility thus induced, the further weakening effects of his erysipelatous attack, it will not be more unfair to presume, than it is encouraging to hope, that, under other circumstances, a more favourable termination would have followed.

With respect to any theory concerning the nature of this disease, and what are termed the *juvantia* and *lædentia* of it, little in the present state of our knowledge can be offered. Nothing hitherto proposed has made even an approach towards an explanation of the manner in which venesection acts upon this disease. The success, indeed, of this treatment, forms a remarkable example of the contrariety of our practice to the most favoured theories concerning this singular complaint, and I am afraid goes far to prove the utter worthlessness of all our theories upon the subject. The practice, in fact, as found successful in the foregoing case, is not only irreconcileable with all former theories upon the subject, but different parts of it are not less irreconcileable with each other. Thus it seemed reasonable to infer that a low diet, comprehending a vegetable food, and abstinence from fermented liquors, would be necessary in aid of venesection; but when this seemingly fair induction was reduced to practice, its falsity became manifest, for while one was observed to subdue the disease, the other was found, in an equally remarkable manner, to increase it. It may be contended, that a vegetable diet must be injurious, from the close affinity which its constituents bear, in its elementary principles, to those of saccharine

saccharine matter, and their mutual convertibility; but if vegetable food, in this way, increase the disease, the difficulty is still unexplained how a copious venesection removes it. The practice of generalizing facts, which is as much too frequently indulged in by some, as it is neglected by others, has perhaps led too many writers to take too simple and too limited a view of the effect of venesection, and to regard its direct influence upon the animal system to be in every case debilitant, and to act as such upon disease. A similar view was formerly very commonly taken of the operation of purgatives; and no inconsiderable degree of praise is due to Dr Hamilton for the solid instruction which his work on purgative medicines has afforded upon this subject *. In fact, venesection and purgatives may operate as tonics in the first acceptation of that term, and not merely as indirectly leading to a state of health and strength, but as directly and immediately conferring it. Every arrangement, therefore, of diseases into the two states of tone and atony, and every attempt at a generalization of facts, deduced from the common and obvious operation of medicines, must be defective, and even mischievous; for however flattering and plausible its simplicity may render it, such a system is founded on false principles, and can only lead to the most erroneous notions of disease, and the most faulty appreciation of remedies.

Hull, November 25, 1813.

VII.

Case of Pneumonia, with remarks on Venesection in that and some other acute Diseases. By Mr Archibald Robertson, Surgeon, R. N.

John Williams, of middle age, short stature, athletic make, and sanguineous temperament, applied for relief on the evening of the 21st October 1812, complaining of acute pain of the

* Des Moyens identiques, (it is justly observed by Bichât,) ont eu souvent des noms différens, suivant la manière dont on croyoit qu'ils agissoient. Deobstruant pour l'un, relâchant pour l'autre, refraîchissant pour un autre, le meme médicament a été tour à tour employé dans des vues toutes différentes et même opposées, tant il est vrai que l'esprit de l'homme marche au hazard quand le vague des opinions le conduit.

the thorax, but particularly fixed in the right side; severe cough, difficult expectoration, with a dyspnœa so violent as to make respiration almost impracticable. The skin was hot and dry; tongue white; belly constipated, and the pulse throbbed with uncommon strength and velocity.

This case occurred during a cruize in the Baltic. The atmosphere for the preceding fortnight had been uncommonly cold, but without moisture. The patient for several days before his application, had laboured under a severe cough and pain of breast; but, like many who indulge an overweening confidence in the powers and habits of health, he had flattered himself that his disease was only *a cold*, and that it would go off spontaneously. Thus he neglected to seek for medical aid, until the complaint had attained the height of exacerbation, and rendered him unfit for even the least laborious of his avocations.

As soon as his actual state had been ascertained, a vein was opened, as usual, at the flexure of the arm. The peculiar urgency of the symptoms called for something of decisive effect; I therefore determined to let the blood flow until the morbid tone of the pulse should be entirely subdued, and the respiration should receive the most marked relief. These objects were not satisfactorily accomplished until more than *forty ounces* had been extracted. There was no unpleasant result from this large bleeding; the patient complained much of nausea and vertigo, and was put to bed with a quick feeble pulse, but his breathing was comparatively free from pain or difficulty.

Early on the following morning a saline cathartic was exhibited. From this time forward his only complaint was a teazing cough, which was soon appeased by the usual mixtures; and in a few days he was discharged to his duty free of complaint, without its having been necessary to repeat the venesection, or to have recourse to any other means than those already stated.

I have not selected this case as being peculiarly uncommon in itself, but rather with the view of introducing some remarks, to which I intend it to serve both as a preface and an illustration.

The connection betwixt diseases and their (intended) remedies has not, so far as I know, been insisted upon by authors, although, to a narrow observer, it is certainly a subject of interesting and important speculation. Were it more attended to in the pathognomonic researches of professional men, I am inclined to think, that many symptoms, and some apparently anomalous ones, which arise during the secondary stages of a dis-

ease,

ease, and which often constitute the principal share of its danger, might be solved to the satisfaction of a practitioner's understanding, although not always perhaps in such a manner as to flatter his self-complacency, or to redound to the honour of his means of cure. The early remedies appear to stand in the same relation to the disease, that *cause* does to *effect* in physics. Those morbid movements of the system which constitute disease, are apt to be stopped, augmented, or differently directed, in the same manner that the motion of external bodies may be increased, arrested, or altered by mechanical agents, favouring or opposing the original impulse. Circumstances often occur where the malady takes its hue from the character of the means first employed, and where its symptoms, whether they tend to recovery or to death, are deducible from the judicious or injudicious treatment adopted at first.

How far the above peculiar connection exists in *all* acute diseases, I leave to be silently decided by every individual who may bestow a few minutes on the perusal of what I am writing, each of whom will no doubt determine, from his own observation, whether the remarks just made are generally or only partially true. But I presume it will strike others as it does me, that, as applicable to pneumonia, they may be believed without reservation.

I have long been impressed with a powerful conviction that the future violence and duration of pulmonic inflammation depend, in a greater measure than is generally suspected, upon the *first* bleeding. If a powerful impression is produced by the abstraction of a very large quantity at first, the disease is suddenly arrested, and will often, in the course of a few hours, be converted from a most violent pneumonia into a simple catarrh; or, if the result is not altogether so fortunate, the symptoms will become infinitely milder and more manageable, and will seldom recur with such violence as to require a repetition of venesection. But the reverse of this picture deserves to be contemplated: If blood-letting has been too long deferred, or from timidity in the practitioner or the patient not largely employed in the first instance, the disease generally proves violent, tedious, and untractable; the patient is harassed by blisters, wasted by antimonials, and broken down by venesections often repeated, which, under such circumstances, prove often mere palliatives, or which at best render convalescence extremely protracted.

An apprehension of the great debility that would be induced by so great a venesection as I here recommend, has, I believe, often restrained the lancet at the moment it is calculated to do most

most good, and the speedy results of this practice have been commuted for the minor advantages of purgative and other medicines. The debility induced by blood-letting, as a general remedy, has, I am persuaded, been too often viewed through the magnifying mist of vague theory, and consequently been much overrated. I am inclined to think, that it is by no means so permanent as that produced by the frequent action of cathartics; at least it is obvious, that the *quantum* of debility produced by one sudden and copious evacuation, is by no means so great as the *sum* of that which is produced by the influence of repeated blood-letting and catharsis, superadded to the languor which always will attend a lengthened illness.

The patient generally bears the first bleeding best, and it is almost inconceivable how great a quantity can be taken at this particular crisis, not merely with impunity, but with singular benefit.

I have met a case of pneumonia where the symptoms were singularly violent, and where I withdrew *forty-eight ounces* at once, before the urgency of the pain and dyspnœa, and force of the pulse, were effectually subdued. The disease was completely *cut short*, and next day his only ailment was slight debility, from which he soon recovered. *

The induction to be drawn from the above observations is, that the efficiency of blood-letting in subduing pneumonia, does not depend by any means so much on the aggregate number of ounces (be they ever so considerable,) drawn during the progress of the disease, but rather on the largeness of the quantity drawn at one time, and that at the commencement. Venesection thus practised, produces a very powerful impression no doubt on the nervous as well as the sanguiferous system; and if syncope is superinduced by it, the morbid agitation of the circulation is at least interrupted, and nature has an opportunity, as it were, of beginning anew, and setting on foot a healthy action.

To a spectator of every-day medical occurrences, it surely must appear an inconsistency in a practitioner, when called at the commencement of pneumonia, to order a limited number of ounces of blood (say 20 or 30) to be drawn at first. Such a practice is more reconcileable to the mechanical habit of extemporaneous prescription, than to views of professional science and usefulness. Bleeding is a remedy whose virtues may be said to be

* It is right to state, that the case here alluded to occurred in a native of a Hyperborean climate, and such individuals are remarked to bear the detraction of blood better than the natives of a more southern latitude, or of our island.

be entirely created by existing circumstances; and it is not unfrequently at the option of the practitioner to give it the greatest activity and efficacy, or, on the contrary, to render it as a remedy null and void. Therefore, in judging of the quantity necessary to be drawn, surely there is an imperative necessity to employ a more rational scale than that of a mere *measure*. In operating upon living subjects, surely the vital powers must form the best marks for our guidance. I wish emphatically to declare, that *the state of the pulse*, and *the cessation* or *mitigation of the most urgent symptoms*, are the only worthy criteria of the quantity to be abstracted. The pulse is the sure index:—let the finger be kept on it during the evacuation, and by means of that "tactus eruditus," which can neither be defined nor communicated by precept, but which every professional man is supposed to possess, sufficient notice will be obtained, and notice too infinitely more authentic than any previous surmise or loose conjectural anticipation of the precise quantity it may be proper to take away.*

I again repeat, that till palpable relief of pain and respiration is felt by the patient, or until his pulse begin to sink, even should syncope supervene, the blood must be permitted to flow. Let us not be deterred by apprehensions of probable asphyxia or other popular terrors, often pertinaciously urged by the patient or his friends. The following observation of Professor Gregory, which is terse and full of intrinsic wisdom, will serve to inspirit us: "The danger of a very large bleeding is less than the danger of the disease."

A practitioner, in thus emancipating himself from the ordinary apprehension and the ordinary rule of practice, will feel that he is performing a great duty to his patient;—he will feel that in his professional department, as in common life, in order, at a particular crisis, to effect some good, it is sometimes requisite to adopt singularity in the *mode*;—that in medicine, as in morals, circumstances now and then exact of him to go

> "Beyond the fix'd and settled *rules*
> Of vice and virtue in the *schools*,
> Beyond the *letter* of the law;"

Well aware of the infallibility of the tests to which he trusts for information, he will resolve for once to be a law to himself.

Copious

* This important remedy is too often put into the hands of an assistant, (with what propriety the *reason* of the thing sufficiently declares,) who contents himself, as indeed is his duty, with drawing the bare quantity prescribed, without any regard to the effect it may have on the symptoms, the pulse, or the general circulating mass.

Copious detraction of blood seldom fails to be followed by powerful diaphoresis; and this effect, too, it produces, without heating the skin and quickening the pulse, as antimonials and the like are wont to do.

It is reasonable to believe, when the disease is not early checked or mitigated, even should gangrene or other fatal termination be prevented, that the organization of the lungs must be deeply injured, from the previous excitement of the circulating system having prevailed for any considerable time. The consequences are perhaps habitual dyspnœa, chronic cough, or even phthisis pulmonalis, one or other of which often conduct or accompany the patient to his grave. How anxious, therefore, ought we to be, out of tenderness for an organ so essential to life, and so susceptible of lesion as the lungs, in the manner above recommended, to attack and overcome the disease *in limine*.

While speaking of the consequences of pneumonia, it is perhaps not irrelevant to state, (what is sufficiently ascertained,) that of late years, since venesection in this disease has been more liberally practised than formerly in the navy, the occurrence of phthisis in our ships of war has been much less frequent.

Cases do not unfrequently occur where this energetic practice unfortunately cannot be adopted on account of accidental circumstances. I have often met with patients where blood cannot be obtained in sufficient quantity *at once* and *at first*, to do good, owing to an idiosyncracy of constitution, which renders them liable to syncope from the mere puncture of the lancet, or from the loss of a small number of ounces, altogether too trivial and limited to affect the morbid force of the circulation. This I always look upon as a singular misfortune to the patient, and it is almost exclusively in such persons that the unpleasant and often incurable sequelæ of pneumonia ever appear.

The mode of practice above recommended, I wish to be understood as particularly applicable to pneumonia; yet there are several other acute diseases where great stress ought to be laid on the *first* bleeding, from the commanding effect it invariably must have on the future progress and termination of the disease. Phrenitis, enteritis, and the like, and perhaps acute rheumatism, are the most prominent of the number. I need not cite sanguineous apoplexy, for it is obvious that this practice is to be adopted without hesitation in a disease which of all others is the most sudden, and requires whatever is done to be done quickly, —a disease whose fell irruption brings to recollection the language of the Roman bard:

"——— concurritur:

" —— —— —— —— concurritur ; horae
Momento, aut *mors* cita venit, aut *victoria* læta."

Here the first bleeding is decidedly of the highest importance, from the consideration that, in all probability, it is the only one which the impetuosity of the disease will leave it in our power to put in practice.

In urging the above facts, I have perhaps bestowed more pains than the occasion requires. There are points both of theory and practice on which professional men differ widely in opinion. Indeed, in one or two instances, at the present day, this difference is so complete as almost to give countenance to those sarcasms which the ingenious *Moliere* used to vent on the faculty of *his* time. But blood-letting in pneumonia, and the acute diseases just enumerated, is a doctrine found in every text-book, and echoed in all the schools ; and were the above remarks nothing more than a simple recommendation of that practice, I should conceive my time misspent in illustrating a *truism*, concerning which there is neither doubt nor dispute. But I have often conceived, that this, like many other important truths which one hears everywhere, is apt to be received and assented to by the mind without that degree of reflection which is indispensably necessary in order to reduce precept to practice. Being *generally* recommended, it often excites only a *general* idea : and when this *general* indefinite idea comes to be acted upon at a *special* crisis, the result too often will be timidity or half-measures, and this solely from an indistinct knowledge of those minor details, without which the main design in any great undertaking will generally fail of success. Aphoristic brevity has its advantages ; yet I cannot help thinking it would be of very great use to the profession, were general precepts, especially when they lead to important practical results, rendered more tangible and more applicable to actual cases, by being frequently unfolded in their full extent, and descanted upon in monographic essays.

Stimulated by all these considerations appended to a subject in itself sufficiently important, I have endeavoured to place the facts that have come within my own observation with regard to venesection in a more striking attitude,—a more imposing view,—by entering minutely into the *modus* ; pointing out the state of the symptoms and the pulse, as the only safe, rational, and infallible tests of the quantity to be drawn ; and with emphasis holding up the peculiar efficacy of one copious and sudden depletion, and that at the first outset of the disease.

To utility perhaps, though not to novelty, the above observations may have some claim. With a certain order of minds, nothing

nothing is relished unless it is new, rare, and striking. But an honest attempt to render any point more clear, or any great remedy more effective, will not be spurned by those of the profession who have felt the painful anxieties of actual practice; who have felt those dilemmas during which even the feeblest aids are thankfully caught up, and who, in the science of medicine as their study, have largely participated in that feeling of difficulty so eloquently expressed (on a higher subject,) by one of the Latin fathers: * "Quantis suspiriis et angustiis fiat, ut, *quantulacunque ex parte*, possit *intelligi!*"

H. M. Ship Cydnus at sea, 1814.

VIII.

Pathological and Practical Observations.

(Continued from p. 65.)

I SHALL now enter more into detail, with a view to illustrate, by practical evidences, the truth of the several foregoing positions, which may appear to have been advanced on theoretical grounds only. And, in order to avert the charge of fitting the facts to the theory, I shall, as far as I am able, adopt the method recommended by Condillac, and give the results of my experience in the order in which it was acquired, leaving to my readers afterwards to decide, whether the theory flowed naturally from the facts, or whether the records of these appear influenced by a preconceived theory. In using this language, however, I mean not to assume to myself the credit of framing a theory or founding a system. That which I believe in, constitutes also, I am satisfied, the creed of many practitioners, whose treatment of diseases could not be otherwise reconciled. Its materials are widely diffused through the history of the science, and are daily presented to the view of all whose vision is unclouded. To open the eyes of those who are yet blind to its truths and advantages, is an attempt to which no discredit at least can attach; and though I may fail in procuring converts from the more popular practice of cordials and stimulants, I may yet perform a service to those who are from principle opposed to this, by displaying,

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* St. Augustine.

playing, in some degree, the strength of the cause they are attached to.

The first subject I have to illustrate, is that condition of constitutional disease which precedes the formation of fever, inflammation, or acute diseases in general. And here let me retrace some of the steps by which I was first led to a practical consideration thereof.

When I first engaged in the practice of physic, I soon found, what all practitioners are doomed I fancy to experience, that diseases presented a very different aspect from those portraits which nosology has presented us with. I found, too, that the articles of medical treatment which led to determinate results were few indeed, and that much of the practice of physic consisted in administering remedies whose operation in the removal of diseases could not be relied on. To such remedies as were deserving of confidence from possessing obvious and well-ascertained powers of affecting the functions of life, my attention became naturally more directed; and it thence appeared, that to remedies which acted on the circulation, and to those which promoted or regulated the alvine evacuations, we are principally indebted for whatever certain agency we were empowered to employ in the cure of diseases. The employment of blood-letting in cases of inflammation affords one of the few instances, wherein a determinate effect is looked for with a high degree of moral certainty. And its efficacy in relieving such, forcibly exemplifies the great advantage of directly applying medical treatment to the source of disease; for whether it acts by withdrawing the stimulus which excites and keeps up inflammation, or allays this by some other operation not explained or understood, still so closely associated are cause and effect in this instance, that, on the employment of blood-letting, we confidently expect that a reduction in the force of circulation will quickly ensue; in other words, that inflammatory action will abate; and in truth, when the depletion is practised in time, and to a sufficient extent, we are seldom disappointed. So assured is our reliance on this operation, that it emboldens us in extending the employment of blood-letting to cases wherein, without the guidance of some such governing principle, we could never think of applying it.

Thus it has oftentimes happened, that in cases of disordered constitution, where no local inflammation appeared to warrant the general evidences of inflammatory fever that presented, I have, on closer examination, been enabled to detect some obscure deep-seated inflammations previously overlooked, because not marked by any urgency of symptoms characteristic of their nature.

ture. The employment of blood-letting was thus clearly indicated; and its efficacy in relieving such cases led me afterwards to employ it whenever similar constitutional symptoms were manifested, even though no local inflammation could be detected; and finally, I was insensibly drawn by the frequent contemplation of such phenomena, and of the varieties they presented, to a closer inspection and more intimate acquaintance with the several stages of disease so lately specified. The particular cases from which my experience in this respect has been derived, are hardly of a nature to bring forward with effect; while the efficacy of blood-letting, in rectifying in them the errors of the constitution, must be liable to all the doubts and uncertainty that ever attach to preventive medicine. I shall try, however, to render the appearances presented to me as familiar as I can, and to prepare my readers for recognizing them when they meet with such in the course of their own practice. In doing this, I shall be obliged to give the general features, as impressed by frequent repetition on my own mind, rather than the individual portraits from which they were derived; and to bring forward the opinions which resulted from an attentive consideration of actual diseases, in preference to the tedious and wearisome detail of minute circumstances which have in course of time given rise to them. And in pursuing this course, I willingly follow the example of a justly celebrated living writer, whose merits, in other respects, I would it were equally in my power to emulate,—I mean Mr Abernethy, who has so ably advocated the propriety of this mode of detail, that, on his authority alone, I must deem myself justified in adopting it.

It appears to me then, that, previous to the occurrence of febrile or inflammatory action in the heart and arteries, there is a notable interval of disease, marked by evidences of inadequate power in the arterial system to carry on the circulation, and of oppressed and irregular action therein variously modified. The pulse, as formerly remarked, is low, oppressed, irregular, intermittent. The order of succession in which these different states of arterial action occur, or the alterations which may take place in them, I am not prepared expressly to state, but, as far as my observations extend, the first stage is that of feebleness; the second of irregularity; and the third of permanently increased action. That these states will successively ensue upon each other in the order here specified, my experience has taught me to believe, and the effect of depletion, in promoting similar changes, I have repeatedly had occasion to witness.

In cases where there was a low and languid circulation, but where I entertained no doubt respecting the cause of complaint,

I have repeatedly taken blood with the effect of increasing the force of circulation. The increase of force thus produced, alternating with intervals of diminished action, has given rise to the stage of irregularity; which, on further blood-letting, has been succeeded by pulsations, full, strong, frequent, and regular. By continuing the same treatment until this state of increased action had again subsided, disease has been radically removed, and the attacks with which particular organs were threatened, have been altogether averted, and the structure of such organs thus preserved unimpaired. I have at the present time under my care a languid female, whose case is instructive. She applied to me about four weeks ago, complaining of much general indisposition, pains in every part of her body, with great languor and weakness, but presenting no appearances of either general fever or local disease. Satisfied of the nature of her complaints, and of the state of constitution in which they originated, I did not hesitate to take blood freely, and the event fully justified the propriety of so doing; for the effects of the successive depletions, both on the state of arterial action and the character of the blood drawn, were extremely well marked, and highly illustrative of the subject I am engaged in discussing. She has now been bled seven times, and to the extent of 106 ounces avoirdupois. As the system became disburdened, its powers increased, and the subsequent blood-lettings were better borne; and, on inquiring, after the seventh, whether she felt her muscular strength impaired by such copious evacuations, her reply was, that she felt much stronger, and capable of more exertion than she was before any blood was taken. When, by the early evacuations, the arterial power became increased, and tendency to local determinations was manifested, and both the head and chest became affected, so equally liable did both these parts seem to undergo disease, that a mere change of position sufficed to make either disease predominate; for, on lying down, the pains of head became severe, while those of the chest abated; and again, on resuming the erect posture, the head became relieved, while the pains of chest returned. So repeatedly did these alternations occur, that no doubt could be entertained respecting the accuracy of the observation. The changes which took place in the blood were as follows: The first drawn was free from buff, somewhat loose in its texture, but in other respects apparently natural. The second, which was drawn in two days after, and to the extent of 18 ounces, exhibited a thin film of bluish coagulated lymph on the surface of the crassamentum. The third had a thick and firm buff on each cup, and all the following displayed, in some or other of the cups, the same buffy coat, with concave surface, and contracted

ted edges. Before each blood-letting, the pulse was oppressed and irregular, which, with the local affections of the head and chest, afforded the indications by which venesection was regulated. Its effect, in every instance, was to afford immediately, and while the blood was yet flowing, decided relief to the pains of both head and side; and subsequently, to restore freedom, regularity, and increased power to the circulation. The affection of head has here proved the more obstinate of the two; and, though materially relieved, has not entirely given way. It is now, however continued, as I apprehend, by causes unconnected with a plethoric state of the blood-vessels, which, I trust, will soon be also removed, but which it would be foreign to my purpose here to enter on.

The mere lowness of the pulse is the character of it which so frequently misleads; and yet if carefully examined, I apprehend it will be found readily distinguishable from a pulse of debility, for if firm pressure is made on the artery, it will be found to resist this, and, on gradually withdrawing the pressure, to rebound against the finger, with a force much more considerable than a casual examination would lead one to expect. And this mode of examination is always valuable, both in ascertaining the existence of increased action in the earlier periods of diseases, and in determining when blood-letting may be safely relinquished, when employed for the relief of active inflammation; for, notwithstanding the abatement of local symptoms, much danger must ever attend a suspension of the necessary treatment, so long as the pulse retains its preternatural force and hardness.

The irregularity of the pulse affects both its strength and frequency. My observations on this subject induce me to believe, that the irregularity of force, or where the artery makes a few strong pulsations, as if by a sudden effort, and again relapses into a state of diminished and oppressed action, indicates a nearer approach to the period of actual fever or inflammation, than the irregularity of frequency only.

Sometimes the chief characteristic of the pulse is preternatural slowness, at which time it conveys the impression of a stream labouring in its course, and with difficulty propelled. This state, I believe to be more or less connected with fulness of blood-vessels within the head, and consequent pressure of the brain, for I have almost uniformly found it accompanied by evidences of determination of blood to that organ. The other symptoms which attend these periods of disease, are such as evince either irregular transmission of blood, or altered sensations. Of the former, are the coldness of the extremities, frequent rigors, general chillness, dry and corrugated skin, variable complexion,

with many others, to which, in time, are superadded the symptoms of such local disease as may supervene.

To the latter belong the whole train of depraved sensations, and nervous feelings, so generally denominated nervous debility, and so frequently treated by cordial remedies. Now as these affections, so oppressive to the patients, admit of temporary relief by the use of cordials and stimulants, while they are perfectly removable by evacuations, I cannot otherwise judge of them, than as proceeding from a deficient supply of nervous influence to the system, consequent to and occasioned by a state of languid circulation within the brain. In the one instance, the power is restored, and the secretion promoted by the direct effect of the stimulant employed; in the other, a correspondent effect is produced by the increased energy which the arteries are enabled to exert, when relieved from an oppressive load by evacuation. If it is admitted, then, that the state here referred to is one wherein the ordinary powers of the arteries are inadequate to the labour they have to perform; that the general functions are, in consequence, languid and enfeebled, and that the brain in particular, from the blood not being duly propelled through its vessels, fails to supply a sufficiency of nervous influence, the want of which occasions depraved sensations of various kinds, it will readily be understood, why exciting the arteries to increased actions may alleviate some of the distress arising from their disability, and for a time appear to cure the disease. It must at the same time, however, be acknowledged, that the more rational mode of restoring the balance would be rather to lighten the labour; for it should be distinctly understood, that the ultimate effect of the natural actions, when extraordinarily excited, in carrying forward the overload, and conveying it to the various secretories, if abstractedly considered, is injurious to the system rather than salutary, and can in no respect be deemed beneficial, save as averting a greater evil. All of which may be readily conceived merely by tracing the progress of this redundant matter through its several changes to its final expulsion. Thus, by some means, a superabundance of nutritious matter is received into the circulation. The ordinary operation of the several secretories cannot dispose of it; the system, oppressed by the load, becomes languid and enfeebled; at length, from causes which I do not pretend to understand or explain, the vessels become excited to increased actions, which for a time alternate with intervals of diminished energy. A state of permanently increased action finally ensues, the effect of which is to convey blood more vigorously to every part where nutritive matter can be disposed of. A necessity is thus induced in every secerning

ing vessel for the increased exercise of its powers; its actions being excited by the unusual quantity of its appropriate stimulus thus transmitted to it. A proportional quantity of excrementitious matter must consequently be displaced by these increased secretions, and be returned into the circulation, which the excretory organs are next to exert their powers in eliminating. Perhaps these increased actions, thus generally excited throughout the frame, produce that state which we denominate fever, which possibly may essentially consist of the train of occurrences here specified. This conjectural explanation, however, I by no means offer, as affording a perfect solution of a problem hitherto deemed so difficult, but merely introduce it as being in unison with all that we know of the healthy actions of the animal economy, and as tending to explain why, in rectifying the disordered state of the constitution now under discussion, we should adopt the safer and more direct means of evacuating the offending matter by an artificial outlet, rather than the circuitous and objectionable course of exciting to increased exertions the natural powers. That the latter means may succeed, and that they oftentimes do so, I have no doubt. And that the process may be sustained without material injury, I am also willing to believe, provided the redundancy has been only casual, its supply not continued, and the constitution unimpaired. Considerable risk, however, must always attend the employment of such mode of relief; for, in the young and robust, it may excite fever or inflammation, and thus instead of relieving, place life in much danger.

On the contrary, in cases where excess is habitual, the constitution broken, and, above all, when the foundation of organic disease has been already laid, then, I have no doubt, that the endeavour to cure by excitement must prove both dangerous and abortive, being sure to exhaust still further the debilitated powers, to aggravate local determinations, and utterly to fail in accomplishing any thing like a natural or effectual crisis. And yet it is in such cases that its employment is most generally recommended, and will longest continue to be employed; for this class of patients, enervated, as they generally are, both in body and mind, will hardly be persuaded to forego their luxurious indulgencies, or the speedy, though deceitful relief to their sufferings, which stimulants often bestow, although sure to entail on themselves much greater eventual misery, together with decrepitude and premature death. Such treatment, I make no doubt, will oftentimes appear to reanimate, and to be instrumental in prolonging life. And, to the bloated or emaciated voluptuary, it may, in the last stages of his sufferings, afford some valuable relief, and enable him to drag on an unenviable existence with more com-

fort

fort than he could otherwise enjoy. If fairly considered, however, as a means of combating disease, its influence, I am confident, must ever be to implant and confirm this in the habit rather than eradicate it, and to shorten the duration of human life instead of extending it. It is surely a further proof of the ineligibility of this mode of proceeding, that, even when its equivocal advantages appear to be greatest, it requires to be continually applied in order to continue these. For, according to well established laws of the animal economy, increased actions in any muscular part, require to be kept up by continued excitement, else they subside, and are succeeded by a proportional degree of collapse or exhausted power. Such stimulant treatment, therefore, as I have been alluding to, when not immediately successful in establishing a natural crisis, must require to be continually renewed to keep up its effect; even admitting that this was really a salutary one, which under such circumstances, and notwithstanding any temporary alleviation that may attend its employment, it can by no means be considered. And it is a strong reason against pursuing such course, that, while it makes no progress in removing the real cause of disease, it occasions considerable risk of bringing on such a state of increased action as shall amount to actual inflammation, which, if it occur with violence, and in an organ essential to life, may prove fatal. And, consistently with this exposition, I have oftentimes been able to trace back the history of acute diseases, which, from the apparent suddenness of attack, were supposed to arise from some casual occurrence or accidental excitement, although really advancing by a regular, though unmarked train of symptoms, originating in fulness of blood-vessels, and assisted to their acmè by well intended, but injudicious medical treatment.

(To be continued.)

PART

PART II.

CRITICAL ANALYSIS.

I.

Expériences sur le Principe de la Vie, notamment sur celui des mouvemens du Cœur, et sur le Siége de ce Principe, suivies du Rapport fait à la Première Classe de l'Institut sur celles relatives aux mouvemens du Cœur. Par M. Le Gallois, Doctor en Medicine de la Faculté de Paris, &c. &c. pp. 368. 8vo. Paris, 1812.

The custom of the Institute, and other learned societies in France, of ordering a report to be made to them by a small committee, concerning papers and works presented to them, enables us to give an account of these productions when they come to be printed, not only more easily, but also more satisfactorily: Because, although these reports be in general too laudatory, they are always written by persons perfectly conversant with the subject, who not only indicate and condense whatever is most original in the manuscripts submitted to them for their opinion, but are also, in many cases, pledges of the accuracy of the facts and experiments adduced by the author, and frequently take the opportunity of displaying their own learning, and of suggesting views different from those of the author.

Of the advantages of this practice, we have an example in the work before us. The committee to whom the duty of reporting upon M. Le Gallois's papers were entrusted, consisted of Percy, Humboldt, and Hallé; and they not only qualified themselves to appreciate duly his merits, by causing him to repeat before them his most interesting experiments, but they also enter more fully into the historical detail of the physiological doctrines upon which they bear, than the author had permitted himself to do. To them we are indebted for the following sketch of the opinions regarding the action of the heart, with which they commence their report.

After

After the discovery of the circulation of the blood was established by Harvey, physiologists began to study the cause and mechanism of the motions of the heart. Passing over the systems of Descartes, Sylvius de la Boe, and Borelli, the opinion of Willis is the first deserving of notice. He derived the nerves belonging to the voluntary muscles from the brain, and those of the involuntary muscles from the cerebellum, and explained the incessant motion of the latter muscles, by asserting, that the action of the cerebellum never ceased, while the voluntary muscles stood in need of repose, because the action of the brain was not continued. This doctrine was generally admitted until the middle of last century. Boerhaave agreed with Willis, but he added two other causes of the motion and rythm of the heart,—the action of the blood of the coronary arteries upon the fibres of the heart, and that of the venous blood upon the internal surface of its cavities. Next Stahl found no difficulty in explaining every thing by means of his Archaeus. In 1752, Haller published his experiments on irritability, which were supposed to prove that the property of contraction is inherent in the muscular fibre. This property, to which he gave the name of *vis insita*, or irritability, requires, however, according to this doctrine, a stimulus to call it into action, and thus to every muscular motion two conditions are necessary, irritability and a stimulus. The irritability only differs in intensity in different muscles, but it does not obey the same stimulus in all the muscles. The nervous power is the natural stimulus of all the voluntary muscles, and it is in exciting or suspending the action of that power upon the irritability of certain muscles, that the will calls any part into action, or leaves it at rest. It is quite otherwise with the involuntary muscles. These obey stimuli of different sorts, which are appropriated to their functions, and totally distinct from the nervous power. Thus the blood is the natural stimulus of the irritability of the heart, alimentary substances of the intestinal canal, &c. From these principles it was not difficult to deduce the principal phenomena observed in the motions of the heart. Thus, these motions are not under the control of the will, because they are independent of the nervous power; they are incessant, because the irritability which produces them belongs essentially to the fibres of the heart, and the blood which excites them is continually brought by the veins, as it escapes by the arteries. The systoles and diastoles succeed each other regularly and alternately, because the stimulus of the blood always produces the systole, which, by expelling the stimulus, permits the diastole, during which the access of new blood reproduces the systole.

Such is a summary of Haller's doctrine of irritability, which,

which, being founded on experiments made by himself and his principal pupils, was very generally received, but also met with some opposers. The facts upon which its supporters rest their opinions, may be reduced to three heads: 1*st*, If the communication between the heart and the brain, which they considered as the only source of the nervous power, be interrupted, the heart continues to act as before. 2*dly*, If the heart be cut out of the body, it continues to act, and in some instances, for a very long time. 3*dly*, The voluntary muscles are excited to action by irritating the nerves belonging to them; but the heart is not affected by irritating the cardiac nerves or spinal marrow, or medulla oblongata. Admitting the truth of these facts, and only those belonging to the third head have been disputed, it has been asked by the opponents of Haller's doctrine, why, if the nervous power has no influence upon the heart, it is provided with nerves, and why it is so particularly under the control of the passions. Haller never explained himself clearly upon these difficulties. Some of his adherents saw the necessity of modifying his doctrines, and of abandoning the opinion generally received, of the brain being the centre and only source of the nervous power. And they supposed, without direct proofs, that that power was generated throughout the whole extent of the nervous system, and could exist independently of the brain, for a certain time, in the nerves of each part. This modification has been most fully developed by Prochaska; but he is unable to explain why the motions of the heart are independent of volition, and yet in subjection to the passions. He has recourse to the ganglions, but is undetermined what functions to assign to them. Sometimes he considers them as a kind of knots or ligatures, tight enough to intercept all influence of the *sensorium commune* upon the heart, in the simple act of volition, but not sufficiently so to resist the violent impulse of the passions. At other times, adopting the opinion of Winslow, Winterl, Johnstone, &c. that the ganglions are so many small brains, he seems to regard the interruption as complete and constant, and that the action of the passions on the heart is through the medium of the eighth pair of nerves. Fontana and some others of his followers absolutely denied that the nerves of the heart were of any use; while Soemmering and Behrends maintained, that the heart has no nerves, and that those which seem to go to it are distributed entirely upon the coronary arteries, without sending a single twig to the proper fibres of the heart. In opposition to this opinion, Scarpa proved, that in the heart the nerves are as numerous, and are distributed in the same manner as in other parts of the body. But the work of Scarpa did not hinder Bichât from deny-

ing that the nervous power had any influence upon the motions of the heart, being principally induced thereto by his having failed to excite contractions in the heart by galvanising the cardiac nerves, although the fact had been established several years before, by the experiments of Fowler and others; as it has been more recently confirmed by those of Nysten.

Thus we see, that neither system explained the motions of the heart satisfactorily; those who admitted the nervous influence as an essential condition, could not account for the continuance of its motions when its communication with the brain was cut off; while those who denied it were equally embarrassed with the effects of the passions. Since the days of Haller, little, if any, addition had been made to our stock of facts, and without more facts further progress did not seem possible; it was therefore necessary to discover new ways of interrogating nature, which has been happily performed by the author of the Memoirs before us.

I. M. Le Gallois had no intention at first of investigating the causes of the motions of the heart. He was a believer in Haller's theory until experiments, undertaken with a totally different view, led him to inquire if, and in what manner, the nervous power influenced the motions of the heart; and his peculiar method of proceeding has enabled him to produce, in the opinion of the reporters, one of the most beautiful, and certainly the most important physiological work since the publication of the experiments of Haller.

The plan which M. Le Gallois pursued in the course of his researches, was to deduce from each experiment all the conclusions which followed from it, and then to seek in direct experiments the confirmation of these conclusions. He was also particular in knowing the exact age of the animal operated, as he found that it greatly influenced the result; and although he repeated his observations on several kinds of animals, he confined himself chiefly to one, that it might furnish a standard, as it were,—and for this purpose he chose the rabbit, as being easily managed, and obtained in sufficient numbers of any age.

M. Le Gallois is of opinion, that, to know in what consists the essence of life, we must be able to discriminate what is the precise condition in the organization of an animal upon which sensation and motion immediately depend. In this inquiry, there are two things to determine,—what is the nature of this condition, and where is it placed? A multitude of facts prove, that the destruction of, or a certain degree of injury to the brain, produces instant death, and that the division of the spinal marrow paralyzes all the parts below the section. But anatomists were in the

the habit of considering the spinal marrow as a mere nervous trunk, and the brain as the focus of nervous power, and as the only seat of the principle of life.

The occurrence of a particular case of midwifery led M. Le Gallois to inquire into the cause of the death of the fœtus by decollation, independently of hæmorrhage. His experiments were made on young rabbits, and he soon observed, that, after decapitation, life continued in the trunk during the same length of time that the animal would have taken of dying by asphyxia; and that, in both cases, this time was shorter in proportion as the animal was older. The only observable difference was, that in the asphyxized animal, each effort to breathe, as indicated by contraction of the diaphragm, and raising of the ribs, was accompanied by gaping, (baillemens); in the decapitated animal, all the inspiratory motions of the thorax instantly ceased, and the gapings alone continued, as in asphyxia. Precisely the same phenomena take place, when, in place of decapitating the animal, the spinal marrow is divided near the occiput.

M. Le Gallois concluded from these facts, that the decapitated animal perished from asphyxia, and only because it could not perform the necessary motions for inspiration. This opinion was easily brought to the test of experiment; and he found that, by means of artificial respiration, a decapitated animal could be maintained perfectly alive for a length of time, varying according to the species and age of the animal.

It is not, however, on the whole brain that the power of respiration depends, but on a very circumscribed portion of the medulla oblongata, situated near the occipital hole, and towards the origin of the eighth pair of nerves. This M. Le Gallois proved by removing first the brain, then the cerebellum, and lastly, a portion of the spinal marrow, without the respiration being arrested, but it instantly ceased when the origin of the eighth pair was included in the portion removed. It was even possible, by sparing that portion of the medulla oblongata, to decapitate some animals, such as salamanders, so that they survived three or four months, and seemed to die at last of mere inanition.

M. Le Gallois, convinced that the principle of sensation and of motion did not reside in the brain, proceeded in his inquiries. He found that if, in a decapitated animal, which was preserved alive by artificial respiration, the spinal marrow was destroyed, by introducing an iron wire the whole length of the vertebral canal, instantaneously the whole phenomena of life ceased irrevocably; if, without decapitating the animal, an opening be merely made into the vertebral canal, near the occiput, and the whole spinal marrow be destroyed, although in this case the brain be

left

left entire, as well as its communications with the trunk, the life of the trunk is instantly and irrevocably destroyed. Lastly, if a rabbit be divided into two portions, each of these, as well as the head, in the preceding experiment, remains alive for a certain number of minutes, but if, immediately after the division, the spinal marrow be destroyed in either of these halves, life ceases instantly in it, while it continues in the other; and if in this latter only a portion of the spinal marrow be destroyed, all the parts which receive their nerves from that portion are struck dead, while those which receive their nerves from the other portion continue to live.

These experiments prove, not only that the life of the trunk depends upon the spinal marrow, but that the life of each portion of it depends upon that part of the spinal marrow from which it derives its nerves. It is easy to prove, that the spinal marrow is the only source of sensation and voluntary motion in the trunk; because, if the heart and abdomen be opened, and the heart, the lungs, the diaphragm, the intestines, in a word, all the viscera be taken out, and if, in addition, the head be cut off, still the trunk lives; but if the spinal marrow be destroyed, in part or totally, it is struck with partial or general death.

It appears then certain, that the life of the trunk has not its source immediately in the brain, or in any of the viscera of the thorax or abdomen, but nevertheless they are necessary for its maintenance; the brain, not only by its connection with the mechanical phenomena of respiration, but also by the dependence of the proper functions of the lungs on the integrity of the eighth pair of nerves, as will appear in a future part of this analysis. With regard to the viscera of the thorax and abdomen, it is evident that their use is confined to the formation and circulation of blood. Those of the abdomen prepare the materials for supplying the waste which the different secretions constantly cause to that fluid. The lungs impart to it its arterial character, and the heart distributes it throughout the body. We now see, that artificial respiration only supplies to decapitated animals the necessary condition of arterializing the blood. But what is the connection between life and the circulation of arterial blood? It is certain that the life is not in the blood, and that it does not consist essentially in circulation; but it is also certain, that the life which subsists after the cessation of circulation, or after the blood has lost its arterial properties, has only a limited duration; and as the brain and spinal marrow are the sources of sensation and motion, we may conclude, that the general life, or the existence of the individual, results from a certain impression of the arterial blood upon the brain and spinal marrow, which, when once

once produced, lasts for a certain time, according to the kind and age of the animal, but constantly requires to be renewed for the continuance of life.

II. From his preceding experiments, M. Le Gallois was led to conclude, that the maintenance of life in any part of an animal depended upon two conditions,—the integrity of the corresponding portion of spinal marrow and its nervous communications, and the circulation in it of arterial blood. To bring this opinion to the test of experiment, he destroyed the lumbar portion of the spinal marrow of a rabbit twenty days old. The posterior extremities were instantly paralyzed, but, to his surprise, as often as he repeated this experiment, speedy death of the whole animal was the inevitable consequence, which could not be retarded by artificial respiration. The same effect was still more speedily produced by the destruction of the dorsal or cervical portions. The result was considerably modified by the age of the rabbit. When under ten days of age, and even sometimes when fifteen, they survived, at least a certain time, the destruction of the lumbar portion. Several at ten, but none at fifteen, survived that of the dorsal portion, while that of the cervical was fatal in most instances even the day after their birth. Each portion of the spinal marrow, therefore, seemed to exercise upon life two distinct modes of action. By the one it constitutes essentially the life of the part which it supplies with nerves, and, by the other, it contributes to the maintenance of life in those parts which derive their nerves from the rest of the spinal marrow. It is owing to the first mode of action, that the destruction of the lumbar portion instantly annihilates life in the posterior extremities, and to the second, that in a short space it induces death in the rest of the body. The first is constant in all kinds of animals, and at all ages; the second varies according to the kind and age, so that the general life depends in a greater degree on the same portion of the spinal marrow when the animal is a little advanced in age.

The problem then is to discover, what is the kind of action which each portion of the spinal marrow exercises upon the life of the other parts. Now, according to the experiments of M. Le Gallois, the life of each part required two conditions only,—the integrity of its corresponding portion of spinal marrow, and the continuance of circulation in it. That the destruction of one portion of spinal marrow does not injure the integrity of the others, is proved both by the effect of the age of the animal upon the result, and by direct experiment. If the spinal marrow of a rabbit of twenty days old at least, be divided between the last

dorsal

dorsal and first lumbar vertebræ, the whole animal lives; but all connection in regard to sensation and motion is destroyed between the fore and hind parts of the animal; if a hind-leg be pinched the whole posterior half is agitated, but the anterior remains quiet, and *vice versa*: in short, there are two centres of sensation and even of volition, if the motions of the hind-legs, when they are pinched, be the effect of volition. When the life of the animal is still vigorous in this state, if the lumbar portion of the spinal marrow be destroyed, the posterior half of the animal is violently convulsed, while the anterior remains perfectly at rest, but nevertheless, in about three minutes, it too is totally deprived of life. It is therefore evident, that, in this experiment, the death of the animal is caused by the destruction of the lumbar portion, without its affecting the anterior portion of the spinal marrow.

It now remains to examine the second condition, whether the general circulation be deranged or stopped by the destruction of the spinal marrow. Till now, M. Le Gallois believed in the Hallerian theory. Of the two principal facts adduced in support of it, the first, that the motions of the heart do not depend upon the brain, ceases to be a proof that these motions do not depend upon nervous influence, by its being established, that the brain is not the only source of nervous influence. The second fact, that, when the heart is cut out of the body, it continues to beat for a long time, is also true; but it has never been ascertained whether these motions are capable of continuing the circulation. Therefore, the question still remains to be resolved, whether the total or partial destruction of the spinal marrow has the effect of suddenly stopping the circulation. Now, the signs of the circulation being stopped, are derived from the absence of hemorrhage when a limb is cut off, from the colour of the blood, when there is any hemorrhage, being venous, and from the state of the carotid arteries. When these arteries are full and round, and the colour of the blood in them changes to a bright vermilion on the respiration being artificially renewed, the circulation certainly continues, but when these arteries are flat, and the little blood they contain does not change its colour by artificial respiration, circulation has ceased. M. Le Gallois also carefully noted the duration of the signs of life when the circulation was suddenly stopped by cutting out the heart, or tying a ligature around the great vessels at its base, and which he had ascertained to be uniform with the same species of animal at the same age. The following table shows the number of minutes that the gaping and the sensibility continued in rabbits of different ages killed in different ways:

Cutting

	Cutting out the heart.		Opening the thorax.		Submersion.	
Ages.	Sensib.	Gaping.	Sensib.	Gaping.	Sensib.	Gaping.
1	14	20	16	30	15	27
5	6	9	12	16	10	16
10	$3\frac{1}{2}$	4	$5\frac{1}{2}$	$7\frac{1}{2}$	$4\frac{1}{3}$	$5\frac{1}{8}$
15	$2\frac{1}{2}$	$2\frac{3}{4}$	$3\frac{3}{4}$	$5\frac{1}{3}$	3	4
20	$1\frac{3}{4}$	$2\frac{2}{3}$	$2\frac{3}{4}$	$3\frac{1}{8}$	$2\frac{1}{4}$	$3\frac{1}{4}$
25	$1\frac{1}{4}$	$1\frac{1}{2}$	2	$2\frac{1}{8}$	2	$2\frac{3}{4}$
30	1	$1\frac{1}{3}$	$1\frac{3}{4}$	$2\frac{1}{4}$	$1\frac{1}{2}$	$2\frac{1}{2}$

M. Le Gallois next proceeded to try the effects upon the continuance of the circulation of, *1st*, the section of the spinal marrow near the occiput; *2d*, decapitation; *3d*, destruction of the whole spinal marrow; *4th*, of the cervical portion; *5th*, of the dorsal; and *6th*, of the lumbar, upon rabbits of different ages, from birth to one month, and every intermediate fifth day; but, to prevent repetition, he has only stated those on the 1st, 10th, and 20th days, which we have attempted to condense in the following table:

	1 day old.	10 days old.	20 days old.
1. Section of Spinal marrow.	circulation continues.	circulation continues.	circulation continues.
2. Decapitation.	ditto.	ditto.	ditto.
3. Destruction of the spinal marrow.	circulation ceases.	circulation ceases.	circulation ceases.
4. Destruction of the cervical portion.	circulation ceases.	circulation ceases.	circulation ceases.
5. Destruction of the dorsal portion.	circulation continues.	circulation ceases in two minutes.	circulation ceases.
6. Destruction of the lumbar portion.	circulation continues.	circulation continues.	circulation ceases in two minutes.

It follows, from these experiments, that the destruction of the spinal marrow suddenly arrests the circulation. Those motions of the heart which remain after its destruction, or the interruption of the nervous influence upon the heart in any other way, and which misled Haller and his followers, are m[illegible]s without force, incapable of supporting the circulation, and [illegible]logous to the motions of other irritable parts on the application of a stimulus, which, in this case, is the arterial blood contained in it.

Therefore,

Therefore, it is from the spinal marrow, and from the whole of it, that the heart derives the principle of its force; and it follows, that each portion of the spinal marrow exercises, in regard to life, two modes of action perfectly distinct,—one, by which it constitutes essentially the life in all the parts which derive their nerves from it; and the other, by which it contributes to furnish to all the organs which derive branches from the great sympathetic nerve,* and especially the heart, the principle of force and life necessary for the performance of their functions.

In further proof of the influence of the spinal marrow upon circulation, M. Le Gallois contrived to keep part of an animal alive after the rest of it was killed by destroying the corresponding portion of spinal marrow, by preventing that destruction from arresting the circulation. This was done by diminishing the amount of the force required in the heart to maintain circulation, in proportion as the powers it derived from the spinal marrow were diminished; or, in other words, by diminishing, by means of ligatures, the extent of the parts to be supplied with blood by the heart, which explains the paradoxical fact, that, in order to keep alive rabbits of a certain age, after destroying the cervical portion of the spinal marrow, we must begin by decapitating them.

III. It being established that the life of the trunk is derived from the spinal marrow, and that, to prolong it, after the influence of the brain is interrupted, it is only necessary to supply the place of the natural by artificial respiration, a question arises: how long may it be prolonged by this process?—Now the brain can only act upon the trunk through the medium of the spinal marrow and eighth pair of nerves, and it is evident that both are annihilated by decapitation. By means of artificial respiration, we can supply the want of that influence which the

* It is from the great sympathetic that the heart derives its principal nerves, and it is only by means of that nerve that it can derive its powers from all the points of the spinal marrow; and, therefore, it is evident that the sympathetic nerve arises from the spinal marrow, and not from the brain, and that the ganglions are not so many little brains forming with it a nervous system, distinct from and independent of the brain and spinal marrow, as Bichât thought. It must also be observed, that there is a real and important distinction between the parts which derive their nerves from the sympathetic, and those which derive them directly from the *medulla oblongata et spinalis*. The first acquire their principles of action from the entire nervous power, and are not subjected to the will in the exercise of functions, which, during life, have only remissions at most. The last have their principle of action from a circumscribed portion of the nervous power; their functions are subjected to the will, are temporary, and can only be repeated after complete intermissions.

the brain exercises over the respiration, through the medium of the spinal marrow; but there is no evidence that, at the same time, it compensates the loss of the influence of the eighth pair, or is able to prolong life indefinitely.

The section of the eighth pair was made the subject of experiment so early as the time of Trajan, by Ruffus of Alexandria, and has since been repeated by physiologists of every age. But the results were contradictory and unsatisfactory, when M. Le Gallois was led to examine the question; and he began by ascertaining the effect of dividing the recurrent branch of each side alone, that he might ascertain those which were peculiar to the rest of the eighth pair. The results which he obtained were unexpected. The loss of voice was the only effect of dividing the recurrent nerves noticed by preceding observers, from Galen down to the present time; but when the experiment was performed on puppies two days old, they died with evident signs of suffocation in about half an hour. As the recurrents supply the larynx, and as the animals made fruitless efforts to inspire, M. Le Gallois suspected that the suffocation was caused by some sudden contraction of the glottis; and accordingly he found, that, after the animal seemed at the point of death, by making an opening into the trachea, the air rushed into the lungs, respiration was restored, and the animal recovered. He afterwards ascertained that the division of the recurrents acted by paralysing the aretynoid muscles, and thereby relaxing the ligaments of the glottis, so that the size of the aperture was diminished. The effect, however, differs very much in degree, according to the age and kind of the animal operated on. In some species, such as the dog, and especially the cat, the diminution of the aperture of the glottis is so great, that they die nearly, if not as soon as if their trachea was tied up. Guinea pigs and rabbits are less speedily affected. As they grow up, the danger is less urgent, and when they have reached a certain age, they suffer little inconvenience from the section of the recurrents.

The following are the principal facts observed from dividing the *par vagum*.

The division of this nerve on one side only is generally not fatal; but its division on both sides is always fatal.

Its division affects, at the same time, the larynx, the heart, the alimentary canal, and the lungs.

The affection of the larynx depends upon the recurrent nerves; and, in general, when the difficulty of breathing becomes very great immediately after the division of the *par vagum*, and death soon follows, as is the case with horses, it is very probable that the contraction of the glottis is the principal cause.

The chief sign of the heart being affected, is a diminution of the fulness and tension of the arterial system; but the effects upon the heart are not easily determined, and whatever they may be, they do not interrupt the circulation, and other functions are mortally deranged before they acquire their full intensity.

The affection of the stomach is in general greater, but varies in degree according to the kind of animal, and even the individuals of the same kind. In Guinea pigs the stomach loses the power of digesting or propelling the food received into it. But there is no very decided morbid change to be discovered, except sometimes a slight state of inflammation, and the speediness of the death, and the symptoms preceding it, preclude all idea of ascribing it to any affection of the stomach.

The symptoms which indicate an affection of the lungs are the most remarkable and constant. The breathing is rendered high and laborious, and becomes continually more affected. Sometimes it is accompanied by the rattling of froth within the breast. The arterial blood becomes gradually darker, and the animal grows cold. After death, the lungs are found tumid, and partly gorged with blood, partly filled with a serous, and often frothy fluid, and they evidently shew that the external air could not penetrate into them, or only in very small quantity.

It therefore appears, that the section of the *par vagum* kills animals by producing asphyxia, which may take place in three ways: 1*st*, By diminution of the aperture of the glottis; 2*d*, Infarction of the lungs with blood; 3*d*, Effusion of a serous fluid into the bronchia; and the death may be caused by one of these, or by two or all of them variously combined.

The time animals survive this operation is various. In rabbits, M. Le Gallois found it to extend from six hours and one-fourth to eighteen and a half; and it is evident that this must be the maximum to which their life can be prolonged by artificial respiration after decapitation; and that, for many reasons, this can rarely be attained. In fact, M. Le Gallois was never able to prolong it more than five hours and a half.

In these memoirs M. Le Gallois has considered the functions of the brain only in regard to its influence on the mechanical act of respiration, and that which it exercises upon the functions of the lungs by means of the eighth pair of nerves. But he by no means denies that it has an influence equally great and necessary on other parts of the body. On the contrary, he ascribes to it the power of *determining* and *regulating* all the acts of the animal functions. Thus, if a person moves his arm, the principle of this movement emanates from the spinal marrow: but it is the brain which wills and directs it. If the brain be removed from a cold-blooded animal,

mal, although it lives several days, and can move its limbs with sufficient force to change its place, it remains where it is, because its movements are without any object. It moves its feet in contrary directions, and if it make one step forward, it makes the next backwards. In warm-blooded animals, the phenomena are the same, but they are not such favourable subjects of the experiment.

But it is natural to inquire, how does the brain regulate the motions of the body, without furnishing their immediate principle? M. Le Gallois supposes that the brain may act upon the spinal marrow, as the spinal marrow acts upon the parts which it animates, and that it thereby not only determines and regulates the motions of the body, but increases the energy of the spinal marrow itself. This intimate connection between the brain and spinal marrow, serves to explain why causes affecting only the brain should paralyze one side of the body; but he remarks justly, that two facts well ascertained, however inconsistent they may seem, do not overturn each other, but only prove that our knowledge is still imperfect, that some intermediate link has still escaped us.

Besides these principal memoirs, this volume contains a supplement, furnishing the details necessary to enable others to repeat the experiments, and notes on the dentition of rabbits and Guinea pigs, and on the loosening of the bones of the pelvis in the latter at the time of parturition, without which the head of the fœtus could not pass out of it.

II.

The Art of Preserving all kinds of Animal and Vegetable Substances for several Years. A Work published by order of the French Minister of the Interior, on the Report of the Board of Arts and Manufactures. By M. Appert. London, 1812. pp. 162.

MAN has somewhere been defined *the cooking animal*; and the definition is eminently characteristic. All other animals, however sagacious, are content with their food, as furnished by nature, while man, even in his most savage state, submits the greatest part of his aliment to some previous preparation. Even

Even in this point of view, the art of cookery is not unworthy of philosophic investigation. Nor can the utility of the art of cookery be for a moment disputed. By means of it, we are enabled to extract nourishment from many substances, naturally incapable of affording it, and thus to promote the highest object of the political economist, the support of a numerous population. But let us not deceive ourselves. Let us frankly confess, that these motives have had no effect in promoting its cultivation. Let us at once acknowledge, that none of us are insensible to the sensual gratification which it affords, and we shall place cookery in its true point of view. We shall, at the same time, discover the reasons why so many *affect* to hold it in contempt, and feel ourselves authorized to bestow upon it a due degree of consideration.

Cookery is still, what pharmacy long was, a mere collection of receipts, without science or order, consisting, commonly, of a farrago, in which no distinction is made between the useful and essential ingredients, and those preposterous and absurd additions with which they are blended, and the mode of composition is described in terms so vague and unintelligible, that they are useless to those who had never seen them put in practice. The improvement of cookery, however, is more easy than that of pharmacy, for we find even in the beginning of the nineteenth century, a Vans Mons opposing reform and simplification in the composition of medicines, on the grounds that "we know nothing of the state of composition of an electuary, or of a mass of pills, or of a compound plaster, and, consequently, nothing of the nature of their medicinal action, and can, therefore, no more substitute, in such compositions, one ingredient for another, than an alkali for an acid, or an earth for a metal, in a chemical composition. We should, on the contrary, as much as we can, restore to their primitive state, those strange (bizarres), receipts, whose credit time has spared, and which possess some virtues which should perpetuate their use." But every gourmand can decide upon the effect of each ingredient, and each step in a culinary preparation, so far as his taste is concerned. In cookery, then, we have nothing to do but to make the experiments, and reduce their results to principles. But it is the very facility with which the art of cookery is improved that retards its science. The former may be carried to a certain degree of perfection, without much progress being made in the latter; and hence its practice is almost exclusively confined to a class of people generally incapable of improving its principles. The work before us is, however, an exception to this position, and, though the production

production of a professional *restorateur*, well deserves the attention of the public at large, as well as of medical men in particular.

It is needless to enumerate all the advantages which may be derived from being able to preserve animal and vegetable substances for a considerable length of time, without any material alteration of the properties peculiar to them in their recent state. Its importance to the health of seamen engaged in long voyages, is alone sufficient to entitle it to the most serious attention of the physician and the philosopher.

The various means by which the decomposition of organic matter may be prevented or retarded, are reduction of temperature, desiccation, mixture with substances counteracting fermentation, and exclusion of atmospheric air.

Fermentation, including putrefaction, proceeds more rapidly in proportion to the increase of temperature, at least within the range of that of our atmosphere, and is altogether checked by its reduction below it.

Hence in tropical climates, animal food is scarcely fit for use the second day after it is killed; hence the immense advantages, a family in the country, even in these temperate climates, may derive by connecting an ice-house with their pantry; and hence, also, the easy and abundant supply of northern cities with animal food during the winter season. But frost can only be used for the preservation of organic substances in certain climates, is not applicable to all substances, and greatly accelerates the putrefaction of bodies which have experienced its effects, when they are thawed.

The presence of a certain proportion of moisture, is also necessary for the putrefactive process, and it is by various processes of desiccation, that we preserve smoked and hung meat, dried fish, fruits, and vegetables, portable soups, and vegetable roots, for a certain length of time. But its application is limited to few substances, and is often precarious. Besides, it hardens the fibre, destroys the flavour, and changes the taste of the article, so that its qualities as connected with freshness are much impaired.

The admixture of antiseptics alters still more the nature of the substance so preserved. Moreover, spices and spirits are too costly to be employed, except for small quantities of luxuries; vinegar can only be employed for some of the fine kinds of fish, and in the preparation of certain articles of seasoning; sugar is applicable only to a few vegetables, must be used in great quantity, and is exceedingly expensive; while salt, which at present is so extensively employed, "communicates," according to M. Appert,

—" an unpleasant acerbity to substances, hardens the animal fibre, and renders it difficult of digestion. It contracts the animal parenchyma. On the other hand, as it is indispensable to remove, by means of water, the greater part of the salt employed, almost all the principles which are soluble in cold water, are lost when the salt is taken away: there remains nothing but the fibrous matter, or parenchyma, and even that, as has been said, undergoes a change."

The total exclusion of atmospheric air, together with the application of a certain degree of heat, forms the basis of the method proposed by M. Appert, of which he speaks in the following terms:

" This method is not a vain theory. It is the fruit of reflection, investigation, long attention, and numerous experiments, the results of which, for more than ten years, have been so surprising, that notwithstanding the proof acquired by repeated practice, that provisions may be preserved two, three, and six years, there are many persons who still refuse to credit the fact.

" Brought up to the business of preserving alimentary substances by the received methods; having spent my days in the pantries, the breweries, store-houses, and cellars of Champagne, as well as in the shops, manufactories, and warehouses of confectioners, distillers, and grocers; accustomed to superintend establishments of this kind for forty-five years, I have been able to avail myself, in my process, of a number of advantages, which the greater number of those persons have not possessed, who have devoted themselves to the art of preserving provisions.

" I owe to my extensive practice, and more especially to my long perseverance, the conviction:

" 1st, That fire has the peculiar property, not only of changing the combination of the constituent parts of vegetable and animal productions, but also of retarding, for many years at least, if not of destroying, the natural tendency of those same productions to decomposition.

" 2d, That the application of fire in a manner variously adapted to various substances, after having with the utmost care, and as completely as possible, deprived them of all contact with the air, effects a perfect preservation of those same productions, with all their natural qualities.

" Before I state the details of my process, I ought to observe that it consists principally,

" 1st, In inclosing in bottles the substances to be preserved.

" 2d, In corking the bottles with the utmost care; for it is chiefly on the corking that the success of the process depends.

" 3d, In submitting these inclosed substances to the action of boiling water in a water-bath (BALNEUM MARIAE,) for a greater or less length of time, according to their nature, and in the manner pointed out with respect to each several kind of substance.

" 4th,

"4th, In withdrawing the bottles from the water-bath at the period described."

For the details of the process, as varied to suit various substances, we must refer to the work itself. It may, however, be worth while to quote an example.

Boiled Meat. (Pot-au-Feu de Ménage.)

"I put a quantity of meat into the pot to be boiled in the ordinary way. When it was three-fourths boiled, I took out one half of it, the bones of which I had already taken off, as I purposed to preserve it. When the meat (the remainder) was completely boiled, I strained the broth, and, after it had become cool, I put it in bottles which I corked well, tied and wrapped up in their several bags. The beef which I had taken out when three-fourths done, I put into jars which I filled up with a part of the same broth. Having corked, luted, and tied up these, and wrapped them in bags, I placed them, and the bottles containing the broth, upright in a cauldron or boiler. I filled this boiler with cold water up to the rim of the bottles and jars. I put the lid upon the boiler, causing it to rest on the vessels within, and took care to surround it with a wet linen cloth, in order to impede, as much as possible, any evaporation from the water-bath. I heated the boiler, and when the water-bath had been made to boil, I kept up the same degree of heat for an hour, and precisely at the end of the hour, let the fire pass into an extinguisher. Half an hour afterwards, I let off the water from the bath, by means of the cock at the bottom of the boiler. At the end of another half hour I took off the lid. An hour or two afterwards, I took out the bottles and jars. (The time of doing this, is however immaterial, and the operator will consult his own convenience.) The next day I besmeared the corks with rosin, in order to forward the bottles and jars to different sea-ports.

"At the end of a year, and a year and half, the broth and boiled meat were found as good as if made the day they were eaten."

The general efficacy of the process is undoubted. We have not only the testimony of the critical author of the *Almanach des Gourmands*, but of various naval commanders, and of a committee of the Society for the Encouragement of National Industry, consisting of no less persons than Guyton-Morveau, Parmentier, and Bouriat, from whose report we extract the following passage:

"We found in the *pot-au-feu* a jelly tolerably rich, with a piece of beef and two pieces of fowl in the middle. Warming the whole with care, to a suitable degree, the soup was found good, and the meat, which was separated from it, very tender, and of an agreeable flavour.

"The *consommé* appeared to us to be excellent; and though prepared fifteen months before, there was scarcely any discernible difference between its then state, and what it would have been, if made fresh the same day.

"The

"The *milk* was found to be of a yellowish colour, resembling that of colostrum or beestings, more thick, as well as sweeter and more savoury than the ordinary milk: a superiority it derives from the concentration it has undergone. It may be affirmed that milk of this kind, though prepared nine months before, may supply the place of the greater part of the cream sold at Paris. What, however, will appear more extraordinary is, that this same milk having been put into a pint bottle which was uncorked a month before, to take out a part of it, and re-corked afterwards with little care, was also preserved, having undergone scarcely any change. At first it appeared to have somewhat thickened, but a slight shaking was sufficient to bring back its ordinary liquidity. I present it here in the same bottle, that you may convince yourselves of a fact, which I should have had a difficulty to believe, if I had heard of it only, without having the evidence before me."

Although M. Appert has said, that, so far as his knowledge extends, no author, either ancient or modern, has ever pointed out, or even led to the suspicion of the basis of the method he proposes, we may remark, not with the view of detracting from his originality, but of confirming the efficacy of his process, that various preserving processes in common use, depend upon a partial application of the same principles. The preservation of fresh eggs by rubbing them over with butter or immersing them in lime-water, appears to be effected by the total exclusion of the air. Green peas and gooseberries are preserved for months by parboiling them in bottles well filled and closely corked; and even in the process of *potting*, by means of which a fresh round of beef cooked in London, may be eaten in the Channel after having been in China, the spices must have much less effect than the action of the fire, and the total exclusion of the air by means of the frozen suet in which it is imbedded. All these processes, however, are expensive or uncertain in comparison of M. Appert's; and if it shall be found that tin vessels with lids accurately soldered on, answer as well as glass jars, the crew of every ship may be supplied with at least a proportion of fresh provisions during the longest voyages.

III.

III.

Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Volume the Fourth. 8vo. pp. 495., with six plates. Longman and Co. 1813.

AFTER the lapse of a twelvemonth, we are again called upon to notice the progressive labours of this active and enlightened society in the publication of another volume of papers, more copious than any which have preceded it, and at least equal to them in the value of its contents. It affords a promise that the plan of laying annually before the public a collection from the communications of each session, which was announced in the last volume, will be accomplished; and will thus open a regular channel for the speedy promulgation of those essays, for which the authors may desire a more permanent record than the fleeting pages of a journal seem to afford.

It will be impossible to convey any adequate idea of the contents of five and twenty essays, occupying nearly 500 pages, in the course of a limited review; but we shall endeavour to present a cursory view of the principal facts.

The volume opens with a paper from the pen of Mr Ferguson, Inspector-General of Hospitals to the Portuguese army, relating to the remarkable contrast in the virulence of syphilis, as affecting the British soldiers and the natives of Portugal—respectively. Among the former the venereal ulceration is extremely violent and untractable by mercury, so that probably "more men have sustained the most melancholy of all mutilations, during the four years the army has been in Portugal, through this disease, than the registers of all the hospitals in England could produce for the last century:" while among the natives, "the disease is very mild; curable for the most part by topical treatment alone, or wearing itself out without the use of any adequate mercurial remedy." The author, in short, expresses his conviction, from much observation, that the primary symptoms of syphilis are curable, in Portugal, among the natives, without mercury; and the constitutional symptoms by the anti-syphilitic woods, combined with sudorifics. The author attempts to explain, from analogies, how a morbid poison may degenerate and wear itself out among a certain people, while to new-comers, of a different habit, it may prove more virulent and severe. This detail of facts is singular and interesting.

The second paper, communicated by Dr Edward Percival of Dublin, exhibits a curious succession of violent hysterical symptoms, connected with various derangements of the digestive organs, which supervened upon a partial paralysis of the face and neck, brought on by exposure to a stream of cold air. It occurred in a girl of seventeen, of a robust and sanguine habit, who retained her florid complexion and flesh through an illness of many months. A mitigation of symptoms was produced by various remedies, according to circumstances; but the free use of purgatives, indicated by extreme torpor of the bowels, though the evacuations were not very foul or unnatural, was invariably beneficial, and ultimately curative, while bad consequences always followed the neglect or inefficacy of them.

The third paper describes a case of violent and repeated spasms in the tongue, mouth, face, and neck, which, after continuing with augmented severity above a fortnight, notwithstanding the active administration of purgatives and antispasmodics, were cured in a few days, by the successive removal of the decaying stumps of several teeth, which had produced a continued fetid discharge from the inflamed gums, and even from the nose.

In the fourth paper, Dr Chisholm briefly relates "two examples of the beneficial effects of mercury in some severe affections of the brain." These cases occurred in women, and seemed to partake of the united character of hysteria and mania, which returned in paroxysms, and were preceded by pain in the hypochondrium, and scrobiculus cordis. All the symptoms were removed by a mercurial course, which occasioned ptyalism.

Dr Bostock has contributed two interesting chemical dissertations, the fifth and eighth of this collection, the one containing "an analysis of the bones of the spine, in a case of Mollities Ossium," the other relating to "the nature and analysis of animal fluids." In the first of these very clear and perspicuous papers, Dr Bostock describes the analytic processes, by which he ascertained that the diseased vertebræ in question contained only 20.25 hundredth parts of earthy matter, the rest consisting of 57½ of cartilage, and 22½ of jelly and oil. Although there is much room for inaccuracy in the estimates of M. M. Fourcroy and Vauquelin, and others, relative to the composition of healthy human bones, Dr Bostock believes that these contain considerably more than half their weight of earthy matter; whereas the vertebra examined "contained in one part one-fifth only, and in another one-eighth of its weight." He then analyzed the earthy part of the bone, and found the 20.25 parts of the whole

to

to consist of phosphate of lime 13.6, sulphate of lime 4.7, carbonate of lime 1.13, phosphate of magnesia .82.

Dr Bostock has laboured successfully in the prosecution of animal chemistry, and the present essay on the analysis of animal fluids, added to those of Dr Marcet and of Professor Berzelius in the preceding volumes of these Transactions, has contributed much to the elucidation of this important branch of chemical science. The object of this paper is to offer some observations on the two primary animal fluids, which are found in the composition of different parts of the animal body, viz. *albumen*, and uncoagulable part of the serum, or the *mucus*, or *muco-extractive* matter. Dr Bostock points out the means of detecting the existence of these matters, and the effects of different re-agents upon them, and concludes with some practical directions as to the mode of analyzing them. It is impossible to abridge an essay of this nature so as to afford any satisfaction to the chemical inquirer. The application of heat, the addition of concentrated acids, alcohol, and metallic salts, are the means of ascertaining the presence of albumen; and, having given in former essays an account of these re-agents, Dr Bostock principally dwells here upon the operations of the oxymuriate of mercury. All his experiments lead him to conclude, "that the united effect of heat and the oxymuriate of mercury is the most effectual means both of detecting the presence of albumen and of removing it." After a full detail of the analysis of the albumen ovi, and some observations on that of the serum of the blood, Dr Bostock gives a tabular view of thirteen albuminous fluids, which he has analyzed.

He then proceeds to the analysis of the *mucous* fluids. "These fluids differ from the albuminous, in being principally composed of a substance which is not exactly similar to any thing in the blood; and on this account they are to be considered as the products of secretion, rather than of transudation, &c." Of four of these fluids, the saliva, the fluid found in the stomach, a peculiar fluid discharged from the bladder, and one obtained from a patient labouring under ascites, Dr Bostock has detailed the analysis. Before he concludes this valuable paper, he observes,

"There is still a third class of animal fluids, which should next come under our consideration, the peculiar characteristic of which consists in their containing particles visible to the naked eye. I have examined a few of these fluids, which have been discharged from tumours situate both in muscular and in glandular parts. One of them which I had an opportunity of examining with a good deal of minuteness, was procured from a confined tumour on the thigh; its basis consisted of an albuminous fluid, and the particles were composed

composed of a substance very similar to spermaceti, both in its physical and chemical properties. Like this substance, it exhibited a considerable lustre, which it communicated to the fluid, so that when it was gently agitated, it gave to it a waved or glossy appearance, not unlike satin. As the fluid part was miscible with water, while the particles were insoluble, they were readily separated, and retained their lustre for some time after being dried. The fluid which Dr Marcet examined from a tumour in the thyroid gland, was evidently of this class. (See Med. Chir. Trans. Vol. II. p. 374.) These three classes of animal fluids, the *albuminous*, the *mucous*, and the *particled*, (if I may be permitted to use so uncouth a term) are all that it seems at present necessary to establish; but I am aware that future discoveries may add new ones, or alter the arrangement of those that exist."

In a supplementary note to this paper, Dr Bostock gives an account of the analysis of a specimen of a *particled* fluid, taken from a tumour situated among the muscles of the thigh. "From these experiments," he concludes, "it appears that the sediment consists of a peculiar substance, the nature of which appears to be intermediate between albumen and wax, and which may therefore be called albumino-cerous matter."

The paper which follows is of considerable length and great interest. It is a contribution of Sir Gilbert Blane, and consists of a retrospect of his long and ample experience, both in public and private practice, with a view to trace the changes that have occurred in the prevalence and fatality of different diseases, and in the modes and success of medical treatment, during his career of observation. The value of such a retrospect may be estimated, when it is stated, that the documents upon which it is founded are notes, which Sir Gilbert has been in the habit of keeping, of all the cases that occurred to him during the greater part of twelve years in St Thomas' Hospital, and in his private practice at all times. A summary of these documents is given in the form of tables, at the conclusion of the paper, including a series of 20 years; namely, the results of the hospital practice from 1784 to 1794, and those of his private practice from 1795 to 1805.

With a view to comparison, the author briefly recites the most remarkable diseases, which have arisen and disappeared in this country in the course of time, such as the leprosy and sweating-sickness,—those which have arisen, but have not disappeared, such as the diseases of specific contagion, syphilis, &c.—those which have prevailed with various degrees of frequency and fatality at different periods,—and lastly, those which have been more prevalent in our own time, than in former ages. The

The diseases of the second and third class, the plague, small-pox, intermittent fevers, dysentery, typhus, scurvy, syphilis, rickets, &c., have been considerably mitigated in modern times, as the bills of mortality testify, in consequence of improvements in diet, cleanliness, and in the various refinements of life, of vaccination, inoculation, &c.; while the increase of luxury, and the greater proportion of the population, who live independent of bodily labour than in any other age, have contributed to increase consumption, gout, dropsy, palsy, apoplexy, lunacy, and generally all those diseases of which the brain and nerves are the seat. According to Dr Rush, the diseases chiefly incident to savage and barbarous nations, are fevers, fluxes, and rheumatisms. "One cause of their being exempt from many diseases," Sir Gilbert remarks, "is probably the loss of all those children in infancy who are weak and sickly; whereas, in civilized times, those who are saved by good nursing and medical skill, become the victims of other diseases in more advanced life. This may be one cause at least of the modern increase of consumption."

The present generation, however, may congratulate itself on its improved condition, our author states, with regard to those great sources of human misery, epidemic and endemic diseases; and he goes on to show that we are indebted for this improved condition to the lights of knowledge, and the consequent diminution of the remote causes, which are comprised under the three following heads,—the vitiated exhalations and secretions of the living human body,—the noxious exhalations of the earth, and depraved habits of life. "The first includes the plague, the specific contagions, typhus, dysentery, leprosy, and the venereal disease; the second consists of intermittent and remittent fevers; the third comprehends palsy and other nervous affections, gout, dropsy, scurvy, and rickets." These three causes are obviously very much subject to human controul, and Sir Gilbert traces many of the improvements which have taken place, especially in London, by which their operation has been materially limited.

The only other general causes, which influence health, are the climate and the fluctuation of the seasons: but

—"there are chiefly five circumstances belonging to the seasons in this climate which affect health. 1st, It is found that, in a severe winter, a much greater number of aged people die, also of those who labour under chronic affections of the lungs, palsy and dropsy, and of young children. 2dly, There is a greater tendency to pulmonic inflammation in the spring months, in proportion to the prevalence of the north-east wind periodical at this season. 3dly, There is a greater tendency to cholera morbus in the end of summer, and be-

ginning of autumn, and this in proportion to the heat of the preceding summer. 4thly, There is a greater tendency to bowel complaints in general during all the autumn months. 5thly, The strength of the wind has an influence on health. Wind is the great ventilator of nature, and its effects have perhaps not been sufficiently appreciated."

After enumerating the peculiarities of certain seasons, that have occurred within the last twenty years, Sir Gilbert comes to this conclusion; that the fluctuations of the weather, except in the rare occurrence of severe winters, do not much affect health in this age and climate; that Sydenham, Huxham, &c. erred in referring to certain *constitutions* of the atmosphere, or to mineral exhalations; and that the principal changes depend, not upon any mysterious and inscrutable changes in external nature, "but on the compound effect of the state of the weather, and the concentration of human effluvia," which was more incident to the times in which these writers lived than to the present.

But if we have reason to be gratified with the improvements in the prevention of disease which have resulted from advancing knowledge and civilization, so Sir Gilbert proceeds to show, from a long detail of facts, that, in hospital practice, a considerable diminution of mortality is now observed. In private practice, such a comparison cannot be made, in the long list of chronic complaints, from the frequent change of medical attendants under such circumstances. Sir Gilbert institutes a curious comparison between the result of the Hippocratic treatment, in which diseases were left very much to the natural efforts of the constitution, and the more active interference of art in modern times. Of 42 cases of acute disease, of which 37 were continued fever, and 5 inflammations of vital parts, described by Hippocrates, the mortality was 21 of the former, and four of the latter; the proportion of deaths in the whole number being 25 in 42,—a proportion far exceeding that of any modern statement. The rate of mortality, both in the hospital and private practice, as exhibited in the tables annexed to this paper, is one in seven, and in several contemporary statements, published by public institutions, it is still less.

We have no room for any observations on the comparative prevalence of particular diseases in the upper and lower classes of society, which Sir Gilbert has entered into at length, nor for any comments on the tables subjoined. At the end of the volume, we find a supplement to this paper, containing the result of an examination of the bills of mortality in London, with a view to ascertain the comparative mortality of measles since the introduction of vaccination. This inquiry was suggested by the

the perusal of Dr Watt's paper on this subject in his treatise on Hooping-Cough, the inferences of which, we endeavoured to shew in our last number, were hasty and premature. There appears to have been an increase in the deaths by measles within the last ten years, but only in the proportion of somewhat less than 2 to 1; a circumstance which may doubtless be explained from the greater number of delicate infants that are preserved, by improved management and by vaccination, through the first years of life, and are more liable to suffer from the first acute disease that attacks them. A very important note is added to this supplement by Dr Stanger, physician to the Foundling Hospital, who ascertained, by an examination of the records of that hospital, that of 131 patients who had undergone vaccination, and afterwards had the measles, 2 only died; while of 131 cases of measles in children who had had small-pox, 11 terminated fatally. Whence it may be inferred, that small-pox produces no predisposition to milder measles, and that vaccination has no prejudicial influence.

The tenth paper is a communication of considerable length and value from the pen of Mr Wardrop, "On the Effects of Evacuating the Aqueous Humour in Inflammation of the Eyes, and in some Diseases of the Cornea." About seven years ago, Mr Wardrop inserted a memoir in this Journal upon this subject, which he had then had but limited opportunities of prosecuting: he now brings forward the results of a more extended investigation, which tend to confirm the opinions he had at that period been led to adopt, and to shew that the operation in question is to be considered as a valuable auxiliary in the cure of many cases of ophthalmia, and that it sometimes perhaps affords the only means of preventing the total destruction of the organ. It is in this light only that the ingenious writer recommends the operation, and he cautions the reader against supposing that he advises it as the sole remedy in any case of ophthalmia. After having stated that little or no pain is occasioned by an incision of the cornea, where it is not inflamed itself, or otherwise diseased; and that the pain produced, when the cornea is inflamed or partially ulcerated, though acute, soon subsides, and the wound speedily heals, without leaving any cicatrix; he concludes,

"In those cases where the practice of evacuating the aqueous humour is judiciously had recourse to, although the operation may create some temporary irritation, yet its good effects will become immediately perceptible, and in most cases will be permanent. The more obvious of these are a more or less considerable improvement in vision, particularly in those cases where there is a cloudiness in the anterior chamber, a complete cessation of the sense of fulness of the eye-

ball,

ball, and pain in the head; and, in some cases, a very remarkable change in the size of the inflamed vessels."

Mr Wardrop attributes the great and immediate relief, which is produced by the evacuation of the aqueous humour in ophthalmia, principally to the sudden removal of *tension*; and hence compares the operation to the division of the gums in dentition, to the opening of whitloes, &c. After some general observations, he proceeds to discuss in a methodical order, the mode of discharging the aqueous humour, and the modifications of opthalmia in which the practice has been found beneficial; namely, in the purulent ophthalmia both of adults and of children, in the gonorrhœal ophthalmia, in cases where the capsule of the aqueous humour is inflamed, where there is abscess of the anterior chamber, in staphyloma, in prolapsus of the iris, in injuries of the eye-ball, and opacities of the cornea. But as we have not yet travelled half through the volume, we cannot enter into any particular detail. Mr Wardrop has illustrated his observations by the relation of seventeen cases, under the respective heads.

The eleventh paper, with its appendix, comprises three cases of injury done to the brain, related by Dr Hutchinson. The principal inference deduced from the dissections is, "that a morbid structure of the brain may exist for a long time, without much inconvenience to the patient, or the slightest interruption to the various animal functions," and that probably "a variety of chronic and other affections of the head are referable to blows or falls received in early life."

A long, elaborate, and able dissertation by Mr Brodie, entitled "Pathological researches respecting the Diseases of the Joints," occupies the thirteenth place in this volume. The author justly remarks, that considerable inaccuracy is to be found in the acceptation of many names applied to these diseases;—that "the terms white-swelling, scrofulous joints, carious joints, &c. have been employed almost indiscriminately;" and that "the same name has been often applied to different maladies, and the same malady has been spoken of under different appellations." Whence he considers it a desideratum in surgery, that a classification should be made of the diseases to which the joints are liable, that the diagnosis of each may be distinctly pointed out, and the appropriate methods of treatment more clearly ascertained. From a considerable collection of cases which have occurred to Mr Brodie's observation in St George's Hospital, he has attempted a classification and pathological history of the diseases in question, which constitutes the paper of which we now speak; and he in a great measure pledges himself to communicate

cate hereafter his remarks on the diagnostic symptoms, and requisite modes of treatment adapted to each.

In five separate sections, Mr Brodie describes as many classes of diseases of the joints, consisting of 1 inflammation, and 2 ulceration of the synovial membrane; 3, cases in which the synovial membrane has undergone a morbid change of structure; 4, ulceration of the cartilages of the joints; and 5, the scrofulous affection of the joints;—which are the diseases of most frequent occurrence. All these he illustrates by many cases and observations; and concludes his paper with a brief notice of four varieties of disease that are less common; viz. inflammation occurring on the articulating extremity of a bone, followed by an abscess which bursts into the joint;—the death, and exfoliation of a large portion of the head of the tibia, and the consequent destruction of the knee-joint;—the formation of loose bodies in a joint; and the effects of gout, where the cartilages are absorbed, and the exposed surfaces of the bone encrusted with urate of soda. As an example of the perspicuity of Mr Brodie's observations, we shall quote his account of the progress of scrofula in the joints.

"The preceding cases sufficiently illustrate the nature and progress of this disease. The morbid affection appears to have its origin in the bones, which become preternaturally vascular, and containing a less than usual quantity of earthy matter, while at first a transparent fluid, and afterwards a cheesy matter is deposited in their cancelli. From the diseased bone, vessels carrying red blood shoot into the cartilage, which afterwards ulcerates in spots, the ulceration beginning on that surface which is connected with the bone. The ulceration of the cartilage often proceeds very slowly. I have known a knee amputated on account of this disease, in which the cartilage was absorbed for not more than the extent of a sixpence. Occasionally, but not often, a portion of the carious bone dies, and exfoliates. As the caries of the bone advances, pus is collected in the joint. At last the abscess bursts externally, having formed numerous and circuitous sinuses. Inflammation takes place of the cellular membrane, external to the joint. Serum, and afterwards coagulable lymph, is effused; and hence arise a puffy elastic swelling in the early, and an œdematous swelling in the advanced stage of the disease.

"Scrofula attacks only those bones, or portions of bones, which have a spongy texture, as the extremities of the cylindrical bones, and the bones of the carpus and tarsus; and hence the joints become affected from their contiguity to the parts which are the original seat of the disease. I have never met with an instance of the alteration of structure, which has been just described, in the cranium, nor in the middle of the cylindrical bones. Examples of this disease occur in almost every joint of the body, but oftener in those of the carpus

and tarsus, than in any other. In one instance which came under my notice, nearly all the joints of the body were affected at the same time in the same individual."

A very perspicuous and interesting communication follows on the subject of "The Cataract," from the pen of Mr Travers, the object of which is, to determine the particular operation which is best adapted to each of the varieties of cataract respectively. Such a discrimination must have been felt as a desideratum by students and operators amid the different recommendations; and Mr Travers has made out the distinctions in a very satisfactory manner. After tracing the various circumstances under which opacities are observed to form, and their peculiarities of situation, he classes them under four heads, according to their consistency of substance.

"The varieties of consistency, colour, and figure, are numerous. With regard to consistency, we have the fluid or milky, the flocculent or fleecy, the caseous or doughy, and the compact or hard cataract. The fluid lens is, I believe, rarely contained in a transparent capsule. The latter, in my experience, has been partially opaque, presenting a dotted or mottled surface. The capsule appears in contiguity with the margin of the pupil, and as if projecting in it, and the opaque spots upon it are most distinguishable when it is viewed laterally. The second usually resembles, in appearance, flakes of snow irregularly heaped, being visibly of a loose and broken texture, and the larger masses intersected by semi-transparent lines: the arrangement is sometimes regular and uniform, being either foliated or radiated: the capsule is sometimes semi-opaque, but more frequently transparent. The third is the cataract of greatest bulk, impeding the motions of the pupil, having a heavy and dense appearance, uniformly opaque, a clouded not a fleecy whiteness, and sometimes a greenish or dirty white tinge. The fourth appears deep-seated, of a brown yellow, or amber colour, most dense in the centre; if entirely opaque, flat upon the surface, over which the iris plays freely. The second and third species are most frequently met with; the first and fourth are comparatively rare."

To the first and second species, which were formerly regarded as incurable on account of their softness, Mr Travers adds, the operation performed by the late Mr Saunders is admirably adapted: to the two latter, the operation of couching or extraction is best suited. He gives his observations at some length upon these respective operations, and concludes by expressing his belief, that the exclusive preference given by operators to one method, and consequently the frequent mal-appropriation of the remedy to the case, has proved the most common cause of failure in the treatment of cataract. A good coloured engraving is annexed,

annexed, containing ten varieties of the appearances of cataract, illustrative of the preceding remarks.

We are under the necessity of noticing briefly some single cases, which, although interesting in their results, are of less importance than the communications which lead to more general conclusions. Such are, for example, the "instance of the good effects of Arsenic in Chorea," related by Mr Martin of Reigate; the case "of symptoms resembling Tic Douloureux, produced by a wound in the radial nerve," by Dr Denmark, of Haslar; the "history of a case of premature puberty" in a female, communicated from Lincolnshire, by Mr Cooper; the "case of contracted wrist, successfully treated," by Mr Hodgson of Lewes; the "history and dissection of a fatal case of cynanche laryngea," by Dr Edward Percival; the "case of a double encysted tumour, the posterior cyst of which, situated deeply between the eye-ball and the floor of the orbit, was attached to, and partly contained a tooth;" and the "case of extravasation of bile into the abdomen, from a rupture of the liver or gall-bladder," by Mr Fryer, of Stamford.

Mr Chevalier has briefly related "two cases of strangulated femoral hernia, attended with some unusual circumstances," which are worthy the attention of the surgeon. In the first case, the sac was encased, as it were, in a cluster of indurated absorbent glands, through which it was necessary to dissect, and which was continued under Poupart's ligament. After the return of the intestine, by the division of this ligament and of the mouth of the sac, several pints of a purulent fluid, mixed with feces, gushed out; the protruded intestine having previously given way for nearly half of its circumference, but without gangrene, as was afterwards ascertained by dissection. The peculiarity of the second case was, that the sac containing the intestine was included within another sac, into which it had descended, so as completely to fill up the aperture, to which it firmly adhered.

Mr Wardrop has related the history of a diseased metacarpal bone, which was removed by an operation, and has described and given a figure of an instrument, adapted for sawing off the extremities or portions of the long bones. The saw here employed was made by cutting away two-thirds of the head of a common trephine, the remainder of which, when the center-pin was fixed in the middle of the bone, cut through the bone in almost a straight line. A fistulous opening had continued to discharge a serous fluid from time to time, when Mr Wardrop saw the patient two years after the operation, and the cavity was still filled up, merely with a soft elastic mass: therefore, though

this

this operation succeeded in preserving the finger, yet the member was considerably shortened, and of little use.

In the twenty-first paper, Mr Charles Bell enters minutely into the investigation of "the muscularity of the uterus," having had opportunities of dissecting that organ in its gravid state, under all conditions. He describes some portions of the muscular structure, which have not been hitherto sufficiently noticed, and traces the particular operation which they effect: he then describes the natural action of the uterus, the action producing rupture, and the action taking place in the case of Cæsarean operation, as illustrated by dissection. The paper is illustrated by a good plate.

A short paper follows on the use of opium in uterine hæmorrhagy, connected with parturition, which is exemplified in two cases, by Mr Stewart. After which we come to a long, elaborate, and interesting communication from Dr Yelloly, relative to "the vascular appearance in the human stomach, *which is frequently mistaken for inflammation of that organ.*" A correct knowledge of the natural appearances, presented by the stomach after death, is of extreme importance, both on account of the pathological doctrines which are likely to be founded upon them, and of the inferences which may be deduced from them, in cases of death under circumstances of suspected poison. Dr Yelloly has, therefore, done an essential service to medicine and medical jurisprudence, by the full and satisfactory investigation of this subject, which is here detailed. The facts brought forward in this paper, are the result of twenty successive dissections made by the author, with a particular view to the state of the stomach, and of the examination of that organ in five criminals, who were hanged, and in whom therefore the appearances of health were likely to be found. In all the twenty individuals, who died from other diseases, and who had had no affection of the stomach while living, the villous coat of the stomach was found highly vascular in particular parts, as about the pylorus and cardia, with two exceptions: in one of these individuals no vascularity was observed; in the other it was obscure. In the five executed criminals, the vascularity amounted to a red or crimson hue. This is extremely well represented by a highly coloured plate, as it appeared in the stomach of Nicholson, the murderer of Mr and Mrs Bonar. These vascular appearances are distinct for a short time only after death, being best marked on the first day, and soon after, but at irregular periods, becoming more obscure. Similar appearances may be easily produced by injecting the veins of the stomach with a red injection. From these statements Dr Yelloly justly infers,

"that,

—"that, in the villous coat of the stomach, appearances of vascular fulness, whether florid or dark-coloured, in distinct vessels, or in extravasations of various sizes, are not to be regarded as unequivocal marks of disease. They occur in every variety of degree and character, under every circumstance of previous indisposition, and in situations where the most healthy aspect of an organ might be fairly expected. They are found in every part of the stomach, but principally in the posterior part of the great end, and in the lesser curvature; and they cover spaces of various extent, but are generally well defined, and terminate abruptly. These circumstances have been confirmed by a great number of other examinations, which I have at various times had occasion to make; and I am inclined to think, that the first series of cases may therefore be regarded as a fair average of the appearances seen in the adult human stomach. In young subjects, I should imagine, there is in general little vascularity seen after death; for in the 16th case, (that of a young man of 22) there was none apparent; and of the bodies of two boys, which I have since examined, one, where death was produced by an abscess, exhibited a very obscure portion, and the other, where the cause of death was a fracture of the occipital bone, shewed none."

Dr Yelloly supposes, that these vascular appearances take place about the close of life, and gives the following rational explanation of them.

"They are wholly venous, as is also the vascularity seen in many other parts of the body after death; the arteries, to which the veins which are so distended correspond, being generally empty. It would therefore appear, that there is a power capable of being exercised in the artery itself, which carries on the blood to the capillaries, or to the veins, after the further supply of fresh blood from the heart is stopped; and that there is thus a species of accumulation produced in the veins which is adequate to the production of the phenomena in question."

Dr Yelloly alludes to the observation of Mr Hunter, respecting the frequent partial *digestion* of the stomach after death, or its erosion by the gastric juice; and he adduces an argument, which appears to us unanswerable, in refutation of that opinion; namely, that if such an erosion had taken place, the minute extremities of the vessels must have been included in the destruction, and on injecting the arteries, an extravasation would necessarily be observed: but in many trials which Dr Yelloly has made with a view to ascertain that point, particularly where the stomach was exceedingly thin, no such extravasation has occurred. He quotes Hoffmann, De Haen, Stoll, Selle, Portal, Frank, and Haller, who seem to have regarded this appearance of fulness of the vessels as a proof of inflammation; and observes, that numerous examples of the same opinion are to be found

found in the writings of other respectable authors. As inflammation of the stomach is perhaps the only visible result of poisons taken into that organ, the discrimination of inflammation from the appearances described in this paper, would be a point of extreme importance; but to this point our author admits we are not yet arrived. The paper before us, however, has brought us one great step nearer the attainment of truth. When coagulable lymph is thrown out in the substance, or on the surface, of the villous coat, (which he believes to be a rare occurrence) the author considers it as the least equivocal indication of previous inflammation.

"But," he adds, "thick mucus sometimes assumes a good deal the appearance of coagulable lymph; and the existence of a coloured coagulum may occasionally be confounded with a coagulated portion of extravasated blood, adhering (as in the case of Nicholson) to the villous coat; which may the more readily occur, when the former is not in quantity sufficiently great to produce well marked thickening. It is also to be observed, that in cases where mineral poisons have been given, the deleterious operation (as appears from the experiments of Mr Brodie) is generally upon the brain and nervous system; and in this case, the inflammation produced on the stomach itself, may be short of that which would produce effusion of lymph."

Dr Yelloly concludes with some observations relative to the redness often mentioned as observed in the stomach of hydrophobic patients. He has been present at the dissection of three persons, who had died of hydrophobia, but is unable to say that there was any palpable difference between the vascular appearance of the villous coat of the stomach in those cases, and that which is ordinarily seen in the human subject, where there was no affection of the stomach during life.

Of the two remaining papers, the first is "An account of the anastomoses of the arteries at the groin," by Mr Astley Cooper, and illustrated by three coloured and eight plain figures. Since the publication of a former communication, describing the new channels for the blood, where the femoral artery has been obliterated for popliteal aneurism, in a former volume of these transactions, Mr Cooper has had an opportunity of dissecting two persons in whom the iliac artery had been tied, and he here details the course and number of the anastomosing branches, by which the circulation was subsequently carried on. He afterwards relates two cases, in which aneurism of the femoral artery at the groin had proceeded to such a length as to render the probability of success from the operation small, but in which it was performed successfully, notwithstanding the most adverse circumstances.

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The volume concludes with another paper from the pen of Mr Travers, which relates to "the ligature of arteries, and the causes of secondary hemorrhage; with a suggestion of a new method of employing the ligature in cases of aneurism." This very accurate experimental essay is founded upon the prosecution of the discoveries of the late Dr Jones, that the permanent obliteration of the canal of an artery is only to be effected by an inflammatory adhesion of its internal surfaces, which takes place in a similar manner as in a simple incised wound of an external part. The causes of the failure of this obliteration, the author shews to be the same as in the failure of union in external wounds; namely failure of the adhesive inflammation from want of sufficient contact; diffused and extending ulceration; and sloughing or gangrenous inflammation. After a train of sound reasoning from a series of cases and well devised experiments, Mr Travers comes to the inference, that these sources of failure may be best avoided, and the most speedy and effectual obliteration of the canal of an artery may be accomplished by a single ligature, such as will produce a simple division of the internal coat of the artery without injury to the external, which should be retained on the artery a few hours, and then removed. Upon obvious principles (namely, as contributing to the causes of failure just mentioned,) he objects to the double ligature, to the division of the artery, and to the passing of a stitch through the artery.

We have thus attempted to lay before our readers as clear a view of the contents of this valuable and multifarious volume, as the limits of our review will admit. Some of the communications, which it contains, must indeed be considered as the records of insulated facts, which it is part of the plan of such a society to collect; but from many of the dissertations a considerable portion of original information is to be obtained; and the volume must be deemed, on the whole, as a valuable contribution to medical and chirurgical science. As the result of the collection of one year, it affords an earnest of the increasing importance and energy of the society, which bids fair to become the common centre of accumulation for the produce of pathological research and experiment.

IV.

IV.

Medical Histories and Reflections. Volume IV. By JOHN FERRIAR, M. D. senior Physician to the Manchester Infirmary, Dispensary, Lunatic Hospital, and Asylum, 8vo. pp. 117. Cadell and Davies, 1813.

THIS spare octavo, as its title imports, is intended as a continuation of Dr Ferriar's former work, but is stocked with much less variety of matter; for, with the exception of half a dozen pages at the end, it contains only two subjects of observation; the one relating to a particular treatment of dropsy, the other diabetes. The author apologizes for the slim appearance of his volume, on the score of the utility of its contents; having discovered, "after many years of research," a more certain *hydragogue*, than any which the faculty have hitherto been accustomed to employ; and having met also with success, in the treatment of many instances of diabetes, of which an unusual number appears to have fallen under his observation. With respect to two such diseases as dropsy and diabetes, the experienced practitioner will not perhaps be very sanguine in his expectations of a remedy of very general power; but he will thankfully listen to the evidence, which a physician of the standing and reputation of the author may bring forward

The hydragogue, which it is the object of the first part of this treatise to recommend, is one of the oldest articles of the materia medica, having been in familiar use in the time of Hippocrates; and the general neglect, into which it has fallen, is attributed by Dr Ferriar to the over-doses in which it was administered. The extract of elaterium is the hydragogue in question; and the author considers it as surpassing every other medicine in the power of removing serous accumulations, and as affording "astonishing relief" in the dyspnœa occasioned by hydrothorax and ascites. The sensible effects of elaterium, he states, "are severe and constant nausea, frequent watery stools, and, in considerable doses, vomiting. It does not uniformly increase the urine; and for this reason, it is generally proper to combine it with more certain diuretics." Dr Ferriar has briefly related twenty cases of hydrothorax, ascites, and general dropsy, in which he administered the elaterium with various success. It seems to have operated beneficially on a considerable majority of these patients, and some, especially half a dozen hospital patients, are reported as dismissed cured. In the private patients who were relieved, the

the dropsical symptoms seem to have often returned, and death at length followed. This, however, is the course to be anticipated in the majority of dropsical cases, which we know to originate most frequently in organic disease. In some patients, the medicine was intolerable in the smallest doses, exciting great disturbance of the stomach; and several complained of its disagreeable operation, in whom it afforded relief.

Dr Ferriar began the exhibition of this medicine in doses of half a grain once a day; but he soon found it expedient to reduce the quantity materially, and for a considerable time employed it in solution, in the proportion of a grain of the extract to a four ounce mixture, the dose of which was a drachm three or four times a-day, taken in a little water. With a view to augment its diuretic power, he combined it with other medicines; composing his mixture according to the following formula:

℞ Extract. elaterii, gr. i.
Spir. æther. nitros. unc. ii.
Tinct. scill.
Oxymel. colchic. sing. unc. ss.
Syrupi rhamni, unc. i. M.

We were a little surprised to find our author entering into an eulogium upon the word *farrago*, and adducing the habitual employment of such combinations by Sydenham, Willis, and Hoffman, as an argument for the soundness of the practice. Upon the same grounds, every prejudice of education or of the age in which he lived, might be defended as rational in the conduct of every man of acknowledged talents. To a certain extent we cannot doubt that medicines of similar power may assist the operation of each other; but surely an extensive combination of substances, even of acknowledged virtues, cannot but tend to obscure our knowledge of the operation of remedies, and to afford a shelter for ignorance and mysticism: nor will the following whimsical hypothesis tend to confirm the solidity of the author's advice. "It may appear to some persons a fanciful idea, but I have been led by my observations to suspect, that there exists, in the relative effects of medicines, something similar to the *harmony of colours and sounds*; and that the impulse requisite to the living powers of the body, which cannot be produced by a single impression, may be effected by a concurrence or succession of impressions, in some measure dependent on each other." p. 42.

Passing to the subject of diabetes, which occupies the largest portion of the work, Dr Ferriar relates very briefly the symptoms of ten cases of the disease, chiefly occurring in hospital practice, and gives, in tables, a daily register of the quantity of drink and of urine, as the principal indication to be attended to. In his

essay on digitalis, if our memory be correct, Dr Ferriar stated, that he had experienced so much success in the treatment of diabetes by means of a combination of uva ursi, cinchona, and opium, in the cases which had come under his care, that he had not deemed it necessary to make any trial of the plan of animal diet, recommended by Dr Rollo. In the ten cases, which are here related, he has uniformly combined the two modes of treatment, prescribing for the patient a dose of the medicine just mentioned four times a-day, and directing him to live entirely on animal food. The dose consists of half a drachm of cinchona, the same quantity of uva ursi, and half a grain of opium, to be taken in lime-water. We did not know whether to impute this combination to the failure of the medicines, or to the author's attachment to *farrago*, until we proceeded to the perusal of an hypothesis, which he has subjoined to this detail of facts, and which seems to explain the proceeding.

We acknowledge that we did not expect to find, in the fourth volume of that clinical collection of experience, the Medical Histories and Reflections of the senior physician of the hospitals of Manchester, a long hypothetical disquisition, which might have adorned one of those ingenious essays, which, annuente summo numine, illuminate the medical horizon of this place twice a year, and are soon forgotten, even by their enamoured authors. But, in truth, the "extreme vessels," which from their minuteness elude our most inquisitive research, and from their obscurity tempt the exercise of our imagination, were never enlisted in a more fanciful service, since their recovery from spasm, than they are here called upon to perform. The extreme emaciation, which goes on during the progress of diabetes, leads Dr Ferriar thus far to "agree with the opinion of Dr Cullen, that the supply of nourishment for the solid parts is interrupted." He is satisfied, however, that it does not take place in the stomach and first passages, nor in the lungs, "because saccharine matter does not exist in any large quantity in the blood of diabetic patients. Where, then, does this interruption of the nutritive process take place? In these convenient extreme vessels.

"The animal body," says the author," is in a constant state of change and renewal, the particles removed by the absorbents being replaced by the action of the extreme vessels. It is the function of these minute agents to convert their contained fluid into muscular fibre, ligament, nerve, or bone. In local diseases they form purulent matter and granulations for the cure of wounds; they reproduce the cutis and cuticle after accidents; unite bones after fractures; and

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appear, from Dr Hunter's preparations, to be themselves converted, upon occasion, into the parts whose formation they have prepared.

"If these vessels should, from any cause, take on a morbid action, and instead of supplying nutritious matter, should form a substance which cannot be applied to renew the waste of the system, the diseased secretion must either accumulate in the intermediate passages of the circulation, where it would produce hectic fever, (by absorption), and subsequently death, or it must be carried off by some of the emunctories, to which its stimulus must cause an increased determination of fluids. This I conceive to be *precisely the case in Diabetes.*" p. 98.

Now, if we could admit this supposition as a fact, and could farther discover that the matter so produced were sugar, we have still such a chasm in the hypothesis as can only be filled up by other suppositions. There is the same difficulty in conveying this saccharine matter to the kidneys, without its appearing in the blood, as in the other hypothesis. In one place (p. 95,) Dr Ferriar conveys it thither by a convenient skip, called *metastasis.* "To this operation, which is well known by the name of metastasis, I am inclined to refer the deposition of the diabetic fluid in the kidneys." But he is obliged to admit, "in what specific manner the process of metastasis is performed, we cannot, in the present state of knowledge explain. The difficulty is not greater in diabetes, than in some other diseases." In another place, (p. 99), he supposes a sort of bye-road in the circulation, by which the general current is avoided, if we comprehend the following passage.

"The matter resembling molasses, which forms so large a constituent part of the diabetic fluid, having acquired a tendency towards crystallization, cannot be applied to the purposes of nutrition. Its presence in the vessels destined to that office, operates as a stimulus from a foreign body; it is therefore *hurried, perhaps by the circuit of anatomosing branches,* to the kidneys, exciting, by its quality, increased action in the whole system of vessels connected with those organs, and is discharged with an effort resembling that which enables the stomach to clear itself of offensive matters contained in it."

It must be obvious, that most of the links in this chain of circumstances are entirely suppositious, and not the result of observation; that one difficulty is shifted off by referring to others; and analogies of very imperfect coincidence are resorted to instead of facts. This is not the proceeding of philosophy, nor such as we expected from Dr Ferriar, in the prosecution of his pathological researches. We shall, therefore, not follow him through the still more vague analogies, which he has attempted to trace between this hypothesis of diabetes, and the exploded hypothesis of Boerhaave, Hoffman, and Cullen, which agree in considering

considering the proximate cause of fever, "as an obstruction *of some kind* in the extreme vessels;" and between the furred tongue, emaciation, and morbid urine of fever, and similar symptoms in diabetes. Dr Ferriar considers that the remedies, which he has found effectual in the removal of the disease, "agree *exactly* with the theory," which he has proposed; and we cannot dispute the exact agreement of a remedy, which acts we know not how, upon a state we know not what, of the extreme vessels, which are beyond our cognizance. Surely, surely, we do not advance a step in the improvement of pathology, by the aid of *suppositions*, however ingenious.

Half a dozen of pages, at the conclusion of the volume, are occupied by the history of a case of scirrhus of the Pylorus, of which the symptoms were unequivocal, during life, and the actual existence was ascertained by dissection after death. The most curious part of the history of these severe organic *diseases* is, the occasional temporary suspension of the symptoms, while the morbid structure still subsists, and which was strongly exemplified in the case here related.

V.

Lectures on Inflammation, exhibiting a view of the General Doctrines, Pathological and Practical, of Medical Surgery. By JOHN THOMSON, M. D. F. R. S. E. Professor of Surgery to the Royal College of Surgeons, and Regius Professor of Military Surgery in the University of Edinburgh. pp. 649, 8vo. Edinburgh, 1813.

THE title of this work sufficiently proclaims the importance of its contents. It is a publication designed for the instruction of students, being the introductory part of a course of lectures on the principles and practice of surgery which the author delivers at Edinburgh; but it also contains some essays and many practical observations which will well repay the attention of any one who will carefully peruse the whole. To get acquainted with the doctrines of inflammation, and to understand the various modifications which that condition of vessels presents in the different textures of the human body, does require more than half an hour's reading, and, therefore, cannot very well be learnt in a pamphlet or in a periodical paper. We think the profession

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at large are under obligations to Dr Thomson for this valuable collection of facts and reasoning, which he has published in so cheap and unostentatious a form; and whilst we admire his erudition and research, we are struck with the candid manner in which he states the want of accurate knowledge on many points, and suggests them as subjects for interesting experiments.

The introductory chapter contains a history of the division of medicine into two departments, physic and surgery; and the author urges, with laudable earnestness, the necessity of all who devote themselves to the practice of the healing art, receiving the same elementary education, and learning the same general principles.

Although the principal object of these lectures is a discussion of the phenomena of inflammation, Dr Thomson has introduced a variety of other important topics; and, like that distinguished pathologist, Mr Hunter, whom he professes to follow as his best and most accurate guide, he has investigated all the morbid actions which arise from inflammation, or in any way are connected with it, and fall under the cognizance of the surgeon. The following abridged list of contents will best show the number and importance of the subjects treated of, and the order in which they are discussed. First, *Definition and Symptoms of Inflammation; Causes of Inflammation; State of the Blood-vessels in Inflammation; Of the Constitutional or Febrile Symptoms of Inflammation; Terminations of Inflammation; Progress and Varieties of Inflammation; Treatment of Inflammation; General and Local Means of Procuring Resolution; Adhesion; Suppuration; Ulceration; Granulation; Ulcers; Hospital Gangrene or Malignant Ulcer; Mortification; Treatment of Mortification; Constitutional and Local Means of Cure; Burns; Frost-Bite.*

The hypotheses respecting the state of the capillaries in inflammation are clearly stated, and the author records a great number of experiments which he has made to ascertain the effects of stimuli applied to the blood-vessels of living animals. He selected for the subject of his observations the vessels in the web of the foot of a frog, as being less liable to the objections urged against some of Haller's experiments; and he found contractions readily excited in the arteries of this animal, by the application of weak volatile alkali or ammonia. On applying with the point of a hair-pencil, a saturated solution of common salt to the arteries in the web of the frog's foot, he uniformly observed, that these arteries, instead of being contracted, as they had always been by the application of the ammonia, were actually and sensibly dilated. A state resembling inflammation, was easily produced by the application of salt, an effect directly the reverse of that which

takes place from the application of ammonia. Of the spirit and tenor of this part of our author's lectures, the following passage will give an example:

"I am inclined to infer, from the results of the experiments which I have related, First, That the velocity of the blood, so far from being always diminished in inflamed vessels, is often increased, particularly in the commencement of inflammation; and that this increase of velocity may continue in the capillary vessels, from the commencement to the termination of that state. This increased circulation occurs, I am inclined to believe, in a greater or less degree, in that state which has been denominated active inflammation. Secondly, That a diminished velocity in the circulation through the inflamed capillary vessels, may take place in the very commencement of inflammation, and may continue during the existence and progress of that state. Thirdly, That this diminished circulation in the inflamed capillary vessels takes place, however, more frequently in the progress than at the commencement of inflammation in healthy and strong persons; and that it is probably a state which occurs in those inflammations which have been denominated passive. This inference I am inclined to believe is warranted by the diminution of velocity produced in the arterial branches by repeated applications of salt, or even in weakened animals by a signal application. If this view of the state of the circulation in inflamed vessels be just, it will follow, that inflammation is sometimes attended by an increased, and at others by a diminished velocity in the circulation through the capillary vessels of the inflamed part, and consequently, that neither of these two states ought to be included in the definition which we give of inflammation." p. 89.

Under the several heads of inquiry, the author has introduced much nice and valuable disquisition on scrofula, on hæmorrhage, and the application of the ligature to blood-vessels, wherein he gives an admirable history of the origin and progress of that practice, and, in another section, a good account of the absorption of bone. We shall select some of the *desiderata* which are thrown out in different lectures, and conclude our analysis with quoting some of the many practical remarks on *mortification* and on *burns*, which we find in the concluding pages of this volume.

"The question, whether blood in any state be capable of serving as a medium into which blood-vessels can shoot, has probably not yet received the investigation which it deserves. Coagulable lymph, modified by the action of the secreting vessels, or, in other words, as the product of the adhesive inflammation, seems to be the common medium of re-union; but we have still to learn, whether, and in what circumstances, the coagulable lymph contained in the blood which is simply poured out from divided vessels, be capable of serving as a medium for blood-vessels to shoot in, or whether some change effected by the action of the vessels pouring out blood be not necessary to

to qualify its coagulable lymph to serve as the medium of re-union." p. 215, 216.

"Have inflamed surfaces or inflamed serous membranes the power of exciting inflammation in sound surfaces, or membranes with which they come into contact? I am inclined to believe that they have; but this is a point which would require to be investigated experimentally." p. 219.

"It is only to be regretted, that we have no very accurate descriptions of the internal surfaces of the external parietes of abscesses or aneurisms, nor, consequently, of the series of changes which take place in these surfaces during the progress of abscesses and aneurisms to the skin. Minute injections, and a careful examination of these parts, will, it is to be hoped, throw some light on this curious process." p. 383.

"No very accurate experiments have, so far as I know, been made with a view to ascertain the temperature of parts in the earlier stages of gangrene. If retarded or obstructed circulation in the capillary vessels be necessary to the state of gangrene, we should, *a priori*, expect a proportional diminution of temperature. But, in ascertaining the temperature of a gangrenous part, we must be careful to distinguish the heat which it receives by communication, from that which is evolved in it by the circulation of the blood." p. 509.

The general and indiscriminate employment of stimulating remedies, both constitutional and local, in all cases of mortification, has often appeared to us injudicious and unsuccessful, and we entirely agree with Dr Thomson in opinion, respecting the advantages of the antiphlogistic regimen, and the use even of evacuations in many instances, where symptoms of mortification begin to shew themselves. It is far more within the power of physical agents to reduce increased excitement, to check over-action, than to rouse new energy, or suspend the tendency to decomposition and death; and we are persuaded that many a patient has been a victim to the effects of bark, wine, opium, and brandy, when a dark coloured speck has appeared on the surface of a wound, or an inflamed tumour.

"The consideration of the distinction of mortification into acute and chronic, the various forms of this affection, the different states of the body in which it occurs, and the variety of causes from which it proceeds, and by which it may be aggravated, must already have prepared your minds to believe that there can be no general plan of cure for mortification, no measures of treatment equally suitable to the differences which occur in the seat, form, progress, and state of this disease, nor to the various morbid affections of the general system by which it is usually accompanied. In attending to the effects supposed to result from the operation of the external and internal remedies which are daily employed for the cure of mortification, there are two facts well ascertained, which appear to me to be peculiarly

deserving

deserving of your regard. The first of these is, that mortification often stops spontaneously without any assistance whatever from medicine; the second, that it often begins and continues to spread, or, even after it has stopt for a while, recommences and proceeds to a fatal termination, in spite of the best-directed efforts of the healing art. Impressed as my mind has long been with the knowledge of these facts, you need not be surprised, if, in treating of the cure of mortification, I should sometimes express myself with doubt, as to the real efficacy of some of those substances, which have at different times been greatly extolled for their virtues in stopping the progress of that disease, and in remedying its local and constitutional symptoms."

Dr Thomson remarks, that he has frequently seen great and sudden relief produced by spontaneous hæmorrhages, in cases, too, where the gangrene had actually terminated in sphacelus, and hence his fears with regard to the use of the lancet in mortification have been greatly diminished. If the pulse be rendered quicker, the skin hotter, and the tongue drier and fouler, by administering stimuli, they are doing mischief, and add to the evil which they are employed to remedy. The Peruvian bark is a medicine completely inert and inefficacious in mortification, according to our author's experience, and this is the opinion, we believe, of the best informed practitioners of the present day.

In the treatment of *burns*, two general methods seem at all times to have been followed in the regular as well as in the empirical practice of the healing art. One of these has for its object, to alleviate the symptoms of burns, by the application of substances which produce a cooling effect; the other to heal burns by calefacients or stimulating substances. Each mode of treatment has its peculiar advantages in the particular cases to which it is adapted, and neither of them ought to be exclusively adopted.

The author follows the division of burns into three kinds or species, first made by Fabricius Hildenus, according to the degree of injury inflicted, or the depth and extent to which they have penetrated. He thus dismisses the controversies about cold and spirituous applications:

"Considerable diversities are required in the local treatment of sloughing burns, according to the stage, degree, and extent of the injury which they occasion, the part or parts of the body which they occupy, and according to the effects which the remedies employed may already have produced. Fashion has often a considerable sway in determining the treatment to be employed in the first stage of these injuries; and if we will but listen to the reporters of cures, there is no want of excellent remedies for this species of burns. My own observation leads me to believe, that the remedies which are usually applied for the first twelve or twenty hours after a burn of the

the third species has been received, have much less influence in preventing fatal effects, and in accelerating or retarding the cure, than has usually been imagined. The severity of the burn, and peculiarities of constitution which manifest themselves only in the progress of the injury, are circumstances, I am convinced, which have much more influence over the immediate and future results in severe cases of burns, than any modes of treatment which you can at first employ. The remedy, however, which has been last and most strongly recommended, is always for the time that which is said to prove most beneficial; and between the supporters of the remedy which is coming into fashion, and the defenders of that which is just going out, very hot disputes, and a great deal of useless altercation, generally take place. The fact is, that most of the remedies for the cure of burns, which are at present, or which have been formerly much employed, seem to possess considerable sanative powers; and that their comparative merits in individual cases can often only be ascertained by a fair and impartial trial. Were I to judge from my own experience, and from the effects which I have seen result from these remedies in the hands of others, I should reckon it a matter of very little moment which of those I am going to mention shall be first employed." p. 600, 601.

Vinegar, either alone or diluted with water, oily and fatty applications, have long been used as remedies for burns; and Dr Thomson speaks in praise of the linimentum ex aqua calcis, in those injuries of the face which are produced by the explosion of gun-powder. This remedy is known in Scotland by the name of Carron-oil, because it is much used in the accidents which occur in the iron-work at the place of that name. Spirituous and resinous substances have been in repute for the cure of burns since the days of Hippocrates and Sydenham, and though sometimes "out of sight, out of mind," they have been neglected, they have been brought into notice again by accident or by interest; thus affording a strong proof of what we have often had occasion to remark, that there is no want of remedies,—for the old writers on physic and surgery are full of remedies for all kinds of ailments, but we only want to ascertain the precise circumstances in which those remedies are useful, and to learn the rules for applying them with success.

VI.

The First Report of the Committee who have undertaken to make Inquiry into and ascertain the extent of the Process practised by Messrs Delahoyde and Lucett, for the Relief of Persons Afflicted with Insanity, and to provide the means of paying the expence of such Inquiry. London. 8vo. 1813. p. p. 19.

JOURNALISTS are like ambassadors, whose duty it is to report all the state papers and transactions which are calculated to inform or to interest their readers. We hasten, therefore, to communicate intelligence which has reached us, and which, if true to half the extent that fame records, would be worthy of a gazette extraordinary,—for it is nothing less than the discovery of a process to restore insane persons to their right reason. The newspapers announced, some months since, that several mad patients had been cured by some secret process, practised by Messrs Delahoyde and Lucett, and the case of Mrs Lancaster, wife of the celebrated Joseph Lancaster (the Count Rumford of schoolmasters, as he has been called) was quoted as a memorable example. A committee, consisting of several royal personages, and many noblemen and gentlemen, have undertaken to make inquiry into, and to ascertain the extent of the means employed by Messrs Delahoyde and Lucett for the cure of insanity; and they have selected four incurable patients from the Government receiving-house at Hoxton, in order to put the process to the severest possible test. Dr Harness (the Commissioner of his Majesty's navy) is one of the present committee, and he, very judiciously, suggested the selection of the most confirmed lunatics from the government asylums. Four patients were accordingly chosen, out of 94, who had been deemed incurable by Dr Weir, Naval Inspector; Mr Haslam, Apothecary of Bethlem Hospital; and Mr Sharp, Surgeon to Sir Jonathan Miles's receiving-house at Hoxton. These four patients, whose names are John Braily, William Matters, James Cardiff, and Daniel O'Keefe, were put under the care of Messrs Delahoyde and Lucett on the 27th of June 1813. The following are extracts from Mr Delahoyde's reports, copied verbatim from the present publication:

"June 30th, Braily's pulse has not exceeded 72 the whole of the day, (before being subjected to the process it was 108, as reckoned by Dr Harness); he has had another good night, and has not been either violent or outrageous; he amuses himself in walking in the garden and playing at drafts."

"From

"From 1st of July to the 7th of August, Mr Delahoyde reported progressive improvement, with a small degree of increased irritation about the period of the change of the moon.

"August 7th, Mr Delahoyde reported that Braily was in a state of convalescence.———

"30th July, Matters's pulse on his arrival was 104; at nine o'clock I commenced the curative process. In the course of an hour his pulse was reduced to 76; at half-past 10 o'clock he fell into a sound sleep, which continued eight hours; he then awoke much refreshed.

"31st, Continued tranquil and sane the whole of the day.

"August 1st, has had another good night, and continues sane.

"2d, His pulse remains at 74, he has had a good night, and I have put him to work in the garden.———

"James Cardiff arrived at eight o'clock P. M. 30th July, with Matters, and underwent the process at half past eight o'clock; his pulse was 98 previous to the operation; at half-past nine his pulse was reduced to 70; he got into a sound sleep, which continued nine hours.

"July 31st, he awoke refreshed and tranquil, and has passed the day in assisting the cook in dressing dinner for the house.

"1st August, Cardiff continues as yesterday, his pulse at 72, and has had a good night.

"2d, Continues to improve, his pulse at 70, and has had another good night.

"7th, The rapid amendment of Matters and Cardiff is more strongly and satisfactorily marked than in any other public case I have had as yet under my care—I consider each of these men in a state of convalescence."———

"I commenced the curative process on Daniel O'Keefe, at half past seven o'clock, the 16th July, in the evening. His pulse, previous to the commencement, was at 98; in the course of 57 minutes after the process, his pulse was at 70, his skin soft, and his countenance serene and tranquil: he has slept seven hours; awoke refreshed, and seemed to have improved in his memory a little—he is at this moment at work in the garden.

"26th July, Works in the garden daily without restraint; his recollection and health progressively improve.

"2d August, His memory daily gets better.

"7th, O'Keefe evidently improves in memory."

Thus it appears, from Mr Delahoyde's statement, that all these four patients, who had been in Bethlem Hospital more than one year, who had been deranged several years, and were obliged to be chained, were brought by his process *into a state of convalescence*, as he calls it, *in the course of six weeks*. The committee, however, do not go so far; they wisely remark, that the violence of the patients' paroxysms were evidently mitigated, and on the 25th of September

September the four men were more calm and collected than at any former time since their removal to Sion Vale. Dr Harness visited these patients three days after the date of Mr Delahoyde's report, accompanied by Dr Weir; and these physicians are too honest and sensible judges to suffer themselves to be imposed upon by a specious change in the symptoms of insanity. They close their report of this visit by saying of two patients, "each of them, after being talked to a short time, reverted to his peculiar complaint, viz. O'Keefe to the loss of his hair, and Matters to his being distended by air." "On my visit of the 25th September (says Dr H.) I could not discover that O'Keefe had made any progress to amendment; Cardiff and Matters I consider improved."—Page 16.

Since printing the foregoing report, Messrs Delahoyde and Lucett have, under a pledge upon honour of secrecy, communicated to their Royal Highnesses the Dukes of Kent and Sussex, and to Dr Harness, their process for restoring insane persons to sanity, and we find a declaration respecting its being quite harmless. No patient has been injured by its application. Has any patient been perfectly restored by it? Before we can venture to send any lunatics from this side of the Tweed, we must ask, *cui bono?* The journey and the change of scene, we know, would render the most furious more calm and collected; and they would not, perhaps, talk the same subjects over and over at Sion Vale as at Edinburgh; but we are doubtful whether any measures would restore them in so short a period as two months; and we shall wait for further evidence before we pronounce any judgment upon the process which is so much talked of. We observe one fault in the general proceeding of the committee. They do not boldly put forward the entire strength of their case; they temporise, they manage, they allow the communication of the nature of the process to some of their members, but not to all. They select too few medical men to form their committee, and they keep secret what they know. This for a committee of inquiry is the choice of a weak point. He has not observed well on the nature of insanity who does not know, that it is intermitting, sometimes periodical, susceptible of change from internal causes, and varying from day to day. In some instances it is apparently fixed and immutable for years. The periods of the changes of the moon have no influence upon mad people, nor is the state of their pulse connected in any obvious way with the state of their minds. Mr Delahoyde particularly specifies the reverse of these two observations; and we own that we are apt to consider any opinions void of truth when we find absurdities connected with them.

PART

PART III.

MEDICAL INTELLIGENCE.

Report of the Diseases treated at the PUBLIC DISPENSARY, *(near Carey-Street,) London, from November* 30, 1813, *to February* 28, 1814.

Acute Diseases.

	No. of Cases.		No. of Cases.
Febris	9	Variola	1
——— infantum rem.	17	Erysipelas	1
Convulsio	3	Urticaria	1
Cynanche tonsillaris	6	Hepatitis	2
——— trachealis	1	Nephritis	1
Catarrhus	195	Podagra	1
Pleuritis	3	Rheumatismus	10
Peripneumonia	45	Arthritis rheumat.	3
Pertussis	12	Abortus	1
Scarlatina anginosa	6	Epistaxis	1

Chronic Diseases.

	No. of Cases.		No. of Cases.
Cephalæa	19	Colica	2
Vertigo	7	Constipatio	3
Hemicrania quotidiana	1	Marasmus	2
Odontalgia	1	Hæmorrhois	1
Catarrhus chronicus	100	Tænia	1
Asthma	1	Ascarides	4
Hæmoptysis	13	Dysuria	5
Phthisis	9	Nephralgia	4
Palpitatio	6	Cystirrhœa	1
Hæmatemesis	1	Menorrhagia	7
Dyspepsia	18	Amenorrhœa	4
Gastrodynia	16	Hysteralgia	4
Pyrosis	1	Prolapsus uteri	2
Hepatalgia	4	Leucorrhœa	1
Icterus	3	Hysteria	8
Diarrhœa	10	Paralysis	6
Dysenteria	12	Asthenia	5
Enterodynia	8	Mania	1

Chronic Diseases continued.

	No. of Cases.
Chorea	1
Rheumatismus chronicus	33
Pleurodyne	7
Hydrops	3
Hydrothorax	3
Anasarca	4
Nodositas	1
Tremor ex hydrargyro	1
Gangrena pedum	1
Struma	4

	No. of Cases.
Lichen	1
Prurigo	1
Psoriasis	3
Purpura	1
Erythema	3
Impetigo	1
Ecthyma	1
Scabies	12
Porrigo	12

Since the severe winter of 1794-5, this country has not experienced such an intense and continued cold, as has occurred during the present season. The early part of December was raw, chill, and occasionally foggy; but the frost commenced on Sunday the 26th of that month. On Monday the 27th in the forenoon, it was accompanied by a thick fog, which in the evening became so dense as to render all objects at a few feet distance from the eye invisible, even by the aid of torches, to prevent the departure of mail-coaches and other carriages, and to occasion numerous accidents in and near London. This singularly dense and dark state of the atmosphere, which was extremely offensive both to the eyes and lungs from being loaded also with smoke and soot, continued for the space of seven days, with scarcely any change, even at noon, except the appearance of a dim light during the latter; and as the frost was likewise uninterrupted, the houses, railings, shrubs, and trees, and even the cobwebs hanging upon them, became thickly spangled with the freezing humidity of the fog. This terminated at the end of a week with a heavy fall of snow, which was repeated, so as to cover the streets to the depth of several feet, and to interrupt the communication with the country for several days. In the last week of January a partial thaw occurred, which brought great masses of ice down the Thames, which, on the recurrence of the frost, became united between the bridges, and in the first week of February was sufficiently firm to bear great multitudes of people, who were entertained in booths erected upon the ice. A thaw of two or three days broke up the ice, but to the end of February, the frost has continued to recur nightly, with partial thaws during the day in the crowded parts of the town. The wind has been almost invariably from the east and north-east, and frequently high, since the disappearance of the fog, which rendered the cold more

more intense to the sensations, than even the thermometer indicated. On many days, however, the temperature was as low as 15° of Fahrenheit, and it was said to have descended, on some nights, to 11° or even lower. The effects of the severity of the cold were aggravated, among the lower classes, by the difficulty of procuring fuel, in consequence of the extreme scarcity of coals, which were sold at more than double their usual prices.

Since Dr Heberden demonstrated the comparative insalubrity of a frosty winter, by a reference to the bills of mortality in the winter of 1794, and the succeeding open season of 1795, medical men have been fully aware of the popular error of considering frost wholesome. It would be almost sufficient, therefore, to inspect the preceding catalogue of diseases, in order to infer the severity of the season. For in the first place, the quarterly number of patients has exceeded 700, in an institution which presents an average of little more than 500 per quarter; and secondly, nearly 400 of that number have been affected with pulmonary complaints, which are the most unequivocal products of the operation of cold on the human constitution. These complaints, however, have not been remarkable for their numbers only, but likewise for their violence and fatality. About 200 assumed the form of acute catarrh, and a large proportion of these are entered in the books of the Dispensary under the title of *febris catarrhalis*, implying the severity of the constitutional disorder which accompanied them; and nearly 50 put on the character of the most active pneumonia. Persons of all ages have suffered from these attacks; but young children and persons who had passed the middle age have been most severely and fatally affected, especially those of the latter who had been subject to annual returns of chronic catarrh during previous winters. The increased mortality, occasioned by severe winters, has generally, indeed, fallen heaviest upon the aged and young children, whose vital powers are possessed of less energy than during the intermediate periods of life; and it is by affecting the lungs more particularly, in both these classes, that intense cold exerts its deleterious influence on the delicate frames of such persons. A cursory inspection of the daily reports of the newspapers, during the last two months, is sufficient to ascertain the unusual number of people of advanced age, who have been cut off by the inclemency of the season.

Very few of the pulmonic affections assumed the type, which has been denominated pleurisy. Severe pain was seldom among the symptoms of the disease; which was characterized by extreme oppression and difficulty of breathing, with a sense

of

of tightness and fulness of the chest, in those which have been put down under the head of peripneumony. This oppression was accompanied, in general, with a rattling noise in the bronchial passages, and with a secretion of a thick and viscid mucus in considerable quantities: the patients breathed more easily when propped up in bed, or in the erect sitting posture; and a sort of wandering delirium, in which they talked incoherently, when left to themselves, was a very common symptom; in a few cases the delirium assumed a character of wildness and violence. The pulse was generally frequent, from 120 to 130, and without either fulness or hardness in any remarkable degree; but, even when small and feeble, there was a perceptible jerk or quickness in its beats, which indicated the inflammatory action of the vessels. For the cure of these peripneumonic attacks the lancet proved the only effectual remedy. In many cases, indeed, the disease proved fatal notwithstanding the employment of blood-letting, especially in people advanced in life, and in those whose lungs were injured by repeated annual attacks of chronic catarrh: and in some, the opportunity for an efficient use of the remedy had passed by before it was applied. We had, however, great satisfaction, in several cases, in witnessing the immediate relief which blood-letting afforded to the most distressing and alarming symptoms: patients, who were restless and sleepless, and extremely distressed with dyspnœa, almost to a sense of suffocation, who were rambling and delirious, and under a state of the greatest anxiety, were seen immediately to become calm and collected, their countenances brightened, their rattling and heaving respiration became free and noiseless, and in a few minutes they fell into a quiet and sound sleep, which continued several hours. The symptoms were, indeed, apt to return, sometimes for several successive periods; but the repetition of the evacuation to the fourth, fifth, or sixth time, was found to be followed by the same temporary, and ultimately by permanent relief. The blood, in these cases, was highly buffed, and the coagulum cupped; and this was sometimes greater in the later bleedings than at first. From the fatality of the disease, where, from neglect in the beginning, or from apprehension of danger from free blood-letting in London constitutions, this evacuation was neglected or insufficiently used, and from the marked and unequivocal benefits derived from the full employment of it, where the age and constitution permitted, we have been led to doubt the correctness of the opinion, which interdicts the most copious abstraction of blood, in the inhabitants of the metropolis, which in the country is universally allowed.

Contagious fever has generally been considered as one of the products

products of a severe winter, in consequence of the close and unventilated state of the habitations of the poor, which the temperature induces them to resort to for the sake of warmth. No such effect, however, has occurred during the present season; the few cases of fever in the preceding list, were of a mild type, and were not propagated by contagion.

A child, two years and a half old, was brought to the Dispensary at half past four in the afternoon, two days ago, who had just swallowed, by mistake, a small quantity (about a tea-spoonful it was supposed,) of the strong sulphuric acid, from a vial, used for cleansing some copper vessels. The mother immediately excited vomiting by putting her finger into the child's throat; the matter brought up resembled coffee-grounds. The child seemed to suffer little pain, except when vomiting, which occasioned crying, and she died easily and almost unperceived at nine the same evening. On the following day, the body was examined in the presence of my friends Dr R. Bright and Dr W. Henderson, who are at present attending the Dispensary, and I am indebted to them for the following report:

"The peritoneal cavity was largely distended with flatus; and on opening the integuments, the omentum was seen converted into a black pulpy mass, but still possessing sufficient tenacity to retain the food, which had escaped from the stomach, having been taken a short time before the acid was swallowed. The food consisted of considerable masses of undigested meat, very little acted upon by the acid; there was also a small quantity of the same dark-coloured fluid in the omentum as had been vomited. There was an erosion or aperture in the stomach, about three inches in diameter, situated in the cardiac portion of the large curvature, which was bordered by thickened edges of a dark brown cinder-like appearance: through this the food had escaped. An appearance of inflammation extended towards the cardiac extremity of the stomach; but the œsophagus, with the exception of a little purple blush on some parts, shewed nothing which could indicate violent action. The inflammation did not seem to extend towards the pylorus, and the intestines were free from any inflammatory affection: externally, the latter were strongly marked with tranverse corrugated rings, but were not otherwise acted upon in the slightest degree by the acid; indeed the whole of its action seemed to be confined to the stomach and omentum."

On immersing small portions of animal flesh in strong sulphuric acid, it produced the same black colour, and pulpy consistence which were observed in the stomach and omentum of this child; but other portions, immersed in caustic potass, a solution of

of nitrate of silver, and nitric acid, did not receive either the dark hue or the same change of texture.

Bloomsbury Square, Feb. 28, 1814. T. B.

Extract of a Letter from Dr BATEMAN.

I have received a long and interesting letter from Mr Magrath, relative to the practice of *cold washing* in measles, which he has been in the habit of employing for a considerable time past, in the hospital for prisoners of war at Plymouth, as I stated in my letter printed in your Journal for October last, on the authority of my friend Dr Lockyer. I regret that my friend Mr Magrath considers this epistolary communication as too hastily written to appear before the public; but, as I trust he will, on revising it, see no reason to withhold it, I shall not anticipate its contents at present, farther than by stating, that he fully confirms the whole of the facts mentioned in my letter. He affirms, that the practice has been uniformly highly beneficial; that he has never seen any of the untoward circumstances which are usually apprehended, such as retrocession of the eruption, increase of the catarrhal and pulmonary symptoms, &c.; and that, on the contrary, the inflammatory affections of the chest, which are apt to supervene, on the decline of the efflorescence, and which prove the most dangerous part of the disease, appear to be prevented by the suppression of the early excitement, to the continuance of which, these supervening inflammations are to be in a great measure attributed. These facts accord so accurately with well established experience, in regard to scarlet fever and small-pox, that we can have no reasonable hesitation in admitting the solidity of the doctrine as applied to a disease of the same family.

I have this day been favoured with the official report of a severe case of measles, in an American sailor, treated on the refrigerant plan, by Mr Magrath; and as it shews that even present pneumonic symptoms are alleviated by the cold-washing, and the subsequent recovery is rapid, I shall transcribe the document of Mr Magrath, and add the testimony of Dr Lockyer as to the decided benefits of the practice, which I also had the satisfaction to receive this morning.

RICHARD GARRISON, a seaman and prisoner of war, was admitted into the hospital of the Mill prison, on the 26th of December 1813. "It appears by a written case, which accompanies this patient, that he has been ill five days, and that the eruption is of two days duration. His eyes are much swelled and inflamed, his throat sore, and deglutition difficult; and he is so hoarse, that he cannot be understood in his attempts to speak. He makes signs that he has severe pain in his chest, which he endeavours to

to alleviate by pressing his hand to the part when he coughs, and which is indeed incessant. He has considerable dyspnœa, and expectorates a thick, viscid, greenish mucus, which is sometimes streaked with blood. His face, chest, and trunk, resemble a boiled lobster; but the eruption appears in scattered patches on the lower extremities. His pulse 110; heat of surface considerable, and such a degree of lassitude, that he could not support himself on his legs, but was obliged to be carried into the ward from the bathing-house, where he was washed with cold water.

"27th.—His cough and pain of chest much abated; his feelings comfortable; and he has slept the greater part of the night. The efflorescence of the skin evanescent, after twenty-five washings yesterday, and six in the night.

"28th.—No complaint but slight hoarseness, and a trifling cough. Was washed fifteen times through the day and night.

"29th.—Convalescent. Has been but five times washed through the day, and not disturbed in the night.

"30th.—Allowed to sit up; and the eruption entirely gone, excepting a *mealy* desquamation or roughness of the skin." On the 3d January he was discharged cured.

The beneficial operation of the cold washings, even upon the catarrhal and pulmonary symptoms, which were unusually severe in this case, was very obvious. In the course of the night, after several washings, he recovered his voice and articulation, and when Dr Lockyer visited him in the morning, at Mr Magrath's request, about 16 hours after his admission, he found him with only a slight hoarseness and moderate cough, without any oppression of anxiety about the chest, and "with evident joy and satisfaction in his countenance, which he also expressed in the strongest language at the wonderful relief he had experienced, in so short a period, from the cold washings." The pulse was reduced to 85; the lassitude gone; and his appetite returning. The eruption was already faded on the face and chest, but Dr Lockyer convinced himself that the eruption was rubeola, by examining it on the extremities, where it was still fully marked and well defined. "I visited him every day," Dr Lockyer adds, "and perceived with pleasure the rapid cessation of cough and hoarseness, and the regular disappearance, and final desquamation of the eruption. He never had, for I watched him ever since this period, any of the dread sequelæ, in horror of which cold air and water have been so long feared and avoided; in proportion as the morbid heat, the pabulum of the disease, was diminished, every catarrhal symptom ceased or greatly abated: here were none of the dreaded effects of repulsion; the eruption

proceeded

proceeded nearly as usual, and fell off in mealy scales about the usual period; but the fury of the disorder was destroyed."

As an individual instance, this case would have been of little weight; but as an illustration of a practice, which has been pursued by Mr Magrath to a great extent, with uniform advantage, it is worthy of attention. A doctrine which is supported both by fact and analogy, will ultimately, no doubt, prevail, when it is viewed without the prejudices attached to the opinions of the schools; for upon such a foundation its truth must ultimately become unquestionable.

February 28, 1814.

Extract of a Letter from Dr BARON *of Gloucester, containing an Account of the last Report of the Central Committee of Vaccination at Paris.*

HAVING been favoured by Dr Jenner with the perusal of the last report of the Central Committee of Vaccination at Paris, I send you a short abstract of its contents, which, I doubt not, you will esteem worthy of being presented to your readers. This report, like the one noticed in your last number, consists of two divisions. The first relates to the administrative measures; and the second to the facts which these measures have been the means of disclosing and collecting. Before I notice the important information thus brought to light, allow me to copy a few of the remarks of the Minister of the Interior, which he delivered in his address to the society, as introductory to the report.

By his testimony, it appears that *one million four hundred thousand children* are annually born in the French empire; that, formerly, one *million passed* through the small-pox in the period just mentioned, of which number *one hundred and fifty thousand* perished. In 1811 there were only *seventy* thousand cases of small-pox, and the deaths amounted to 8500. Thus, he observes, nearly *one hundred and fifty thousand* lives are annually preserved to the empire, and a number, at least, as large, snatched from the numerous evils which follow small-pox.

The assiduity and perseverance with which the administrative measures are executed, do the greatest honour to all who are connected with this grand association. The number vaccinated in 1811 is double that of 1810; but the astonishing progress of the practice will be best illustrated by the following table, which appears at p. 50 of the report. The society for the extinction of small-pox in France was formed in 1804. By its records it appears, that, from April to September 1804 inclusive, 97.151 vaccinations took place in 32 departments.

During

During the end of 1804 and the whole of

1805,	125.992	vaccinations took place in 42 departments.
1806,	149.539	65
1807,	224.249	70
1808,	368.405	89
1809,	269.367	73
1810,	864.016	79
1811,	712.151	125
Total,	2,310,870	

This immense number, it is to be observed, does not include those vaccinated in France between the period when the practice was introduced in 1800, and that when the society was formed. Still further to illustrate the influence of vaccination upon population, I quote the following facts, which are furnished by the department of Sarre. In 1809 there were 4533 vaccinations and 592 deaths occasioned by small-pox. In 1811 there were 7896 vaccinations, and 27 deaths by small-pox,—that is to say, there was an increase of vaccinations to the number of 2353, and a diminution in the number of deaths amounting to 565.

The next part of the report which I shall notice refers to the influence of cow-pox in curing certain chronic disorders of the lymphatic system and the skin. It will be a singular illustration of the obstinacy and folly of human prejudices, should it be found that this practice, which was falsely accused of filling the human constitution with beastly humours, is a powerful and useful agent in removing some unmanageable complaints, which it was formerly supposed to have generated. Dr Jenner has clearly and distinctly referred to this influence in the following paragraph of his first paper on the *variolæ vaccinæ*. "As we have seen," he observes, "that the constitution may at any time be made to feel the febrile attack of cow-pox, might it not, in many chronic diseases, be introduced into the system, with the probability of affording relief, upon well-known physiological principles." In a note, he remarks, that inoculation, by a simple puncture on the arm, will seldom produce this effect. When the disease takes place among the dairy people, the virus comes in contact with pre-existing sores, which does not fail to affect the constitution generally. The importance of these observations has been amply confirmed by observation in this neighbourhood, as well as by the facts which I am about to copy from the French report.

M. Faivre of Saint Lainan, in the department of Jura, vaccinated a child of five years old, which had long laboured under scrofulous ophthalmy. Its bowels were in a very disordered state, its skin dry and scaly, and the neck filled with tumours, of

of which one was in a state of suppuration, and another approaching to it. The vaccine lymph was inserted by many punctions into the fore-arm and the legs. The vaccination proceeded regularly. The eyes became less inflamed, the tumours diminished, that which suppurated almost ceased to discharge, and the skin acquired its natural feel and softness.

M. Ireilles of Moulares vaccinated a girl of five years of age, who had a large scrofulous tumour at the angle of the lower jaw from the time she was 15 months old. The tumour had suppurated at various points, and there was a puriform discharge from the ear. Six punctures were made in the arm, and eight round the tumour. The cure was complete in two months, without the aid of any other means.

M. Missa of Soissons reports the case of a child aged 10, affected with indolent swelling of the cervical and submaxillary glands, and with an habitual scrofulous ophthalmy. It had been thrice vaccinated without success. A fourth attempt was made. The inflammation extended to the shoulder, to the arm-pit, and to the anterior part of the chest. A smart fever accompanied the inflammation; but, as the vaccination advanced, the enlarged glands diminished, disappeared at last entirely, and ten months afterwards M. Missa reports that the ophthalmy was cured without any return.

M. Bompard of Epinal made a curious experiment upon a child four years old, affected since its birth with scaly tetters on the arm, the chest, and the face. He scraped the surface of the tetter, and excited redness of the epidermis; he then applied his finger, impregnated with the vaccine virus, to the part. Three pustules appeared, and the tetters were cured without any other treatment. Four other observations are recorded by M. Serrieres of Nancy, M. Thomas of Roche-en-Berny, M. Gateron of Aigle, and by M. Petiet of Gray. The two last were cases of tetter; the two first of scrofulous enlargment of the glands. It now remains to mention some very singular circumstances regarding the effects of cow-pox in hooping-cough.

M. Amard of Lyons reports, that a child of eighteen months affected with a violent hooping-cough, was vaccinated with eight punctures. On the appearance of the eight pustules, the hooping-cough sensibly diminished; on the eighth day, the child had only one or two paroxysms; on the fifteenth, nothing but a slight cough remained; and shortly after it was completely cured. M. Missa of Soissons vaccinated, at the same time, seven children affected with hooping-cough. With all, the disease was arrested at the moment of the eruption of the pustules; the disease recurred, but in a very mild degree, in two, but in the other five, who had been affected from fifteen to twenty days, it did not

recur

recur at all. Similar facts have been observed by M. Bertrand of Pont-du-Chateau, by M. Jung of Creusnach; M. Fauconier of St Gautier, and M. Creyton of Ause.

I will only add one more case, from this neighbourhood. A clergyman, who for some years had been affected with an intolerable itchy scaly tetter on the left arm, just on the cicatrix of his small pox inoculation, thought, while vaccinating some of his own parishioners, of trying the effect of the virus upon himself. He inserted the matter upon the tetter. A considerable degree of inflammation ensued; the vaccine pustule ran through its course, and the affection of the skin was completely cured. This took place three years ago, and he has remained well ever since. Experiment must decide the best method of exciting the vaccine irritation on the constitution of those who have previously had the disease, or gone through the small-pox. The following observation however just, communicated to me by Dr Jenner, may be worth attending to in this curious inquiry. "From the irritating effects of the cow-pox, when produced by the cow on the milkers, I have remarked that its influence is far greater when the pustules appear on the fingers, especially on the thin skin of the joints, than on any other parts of the hand."

Before I conclude this long letter, allow me to advert to the strange opinion of Dr Watt, noticed in your last number. I have made it my business to inquire into the comparative mortality of measles in this city, and the populous districts in its neighbourhood, where vaccination was first practised. I have reports from Berkeley, from Dursley, from Easlington, from Cheltenham, from Stroud, and from Newent, and not a fact gives the slightest support to Dr Watt's conjecture. Last year measles assumed an alarming type in Newent and its vicinity, but it was equally severe among those who had had small-pox, as among those who had been vaccinated; and I have to add, that the only fatal case of measles that I have seen in this neighbourhood, occurred in a young farmer who was strongly marked by the small-pox.

Gloucester, March 5, 1814.

New Publications.

We have to return thanks for the following Works, presented to us since the publication of our last Number.

An Account of a successful method of treating Diseases of the Spine, with Observations and Cases in Illustration. By Thomas Baynton of Bristol. 8vo. 5s. 6d.

A Treatise on Hernia, by Antonio Scarpa, Professor of Clinical Surgery in the University of Pavia. Translated from the Italian, with Notes and an Appendix. By John Henry Wishart, Member of the Royal College of Surgeons, and one of the Surgeons to the Public Dispensary of Edinburgh. Illustrated by fourteen Engravings. pp. 548. 8vo. 16s.

An

An Essay on Medical Economy, comprising a Sketch of the State of the Profession in England, and the Outlines of a Plan calculated to give to the medical body in general an increase of usefulness and respectability. pp. 160. 8vo. 6s.

The Medical Guide for Tropical Climates; particularly the British Settlements in the East and West Indies, and the Coast of Africa; containing ample instructions for the Prevention and Cure of the Diseases of these Climates: and also on the Voyage Outward and Home. With a Copious Tropical Dispensatory. To which is added, a System of Regulations for the Conduct of Europeans Abroad, in respect to Dress, Diet, Exercise, Sleep, &c. Concluding with a View of the Consequences to their Health on their Return to Europe, after a long Residence in Tropical Climates, and the Precautions on this Head. With Remarks on the Climate and Diseases of different Countries of Europe. By Richard Reece, M. D. Member of the Royal College of Surgeons, Author of the Medical Dictionary and Guide, &c. &c. pp. 237. 8vo. 9s.

Facts and Observations relative to the Fever commonly called Puerperal. By John Armstrong, M. D. Member Extraordinary of the Royal Medical Society of Edinburgh, and one of the Physicians to the Sunderland Dispensary. pp. 162. 8vo. 7s. 6d.

Particulars of the Successful Treatment of a Case of Hydrophobia; with Observations, &c. By Rice Wynne, Apothecary, Shrewsbury. pp. 32. 8vo. 1s. 6d.

There have also been Published,

The Art of Preserving the Sight, unimpaired, to an extreme Old Age. 12mo. 4s. 6d.

A Treatise on the Hot, Cold, Tepid, Shower, and Vapour Baths. By John Land. 12mo. 7s.

Anatomical Instructor. By Thomas Pole. 12mo. 7s.

Observations on the Nature and Treatment of Consumption; addressed to Patients and Families. By Charles Pears, M. D. F. L. S. 8vo.

Practical Remarks on Diseases resembling Syphilis, with Cases. By John Whitsed, Surgeon, Peterburgh. 8vo. pp. 50.

An Examination of the different systems of Shoeing the Feet of Horses, particularly the Thin-Heeled System of the College, and the System now Practised in the Prince Regent's Stables. By R. Powis, Veterinary Surgeon. 8vo. 2s. 6d.

Communications have been received from Dr Clarke, Sidmouth; Dr Arnold, H. M. S. Alcmene; Mr G. N. Hill of Chester; Mr Penkivil, Plymouth-Dock, and Mr Robertson, H. M. S. Cydnus, &c.

*** Communications may be addressed to the Editors, to the care of Messrs CONSTABLE & Co. Edinburgh; Messrs LONGMAN, HURST, REES, ORME, & BROWN, London; and GILBERT & HODGES, Dublin.

ERRATA.—Vol. X. p. 89, line 16 from bottom, *for* convict *read* convert. line 9, ditto, *for* new station *read* nine stations.

No. XXXIX. will be Published on the 1st of July 1814.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. JULY 1814.

PART I.

ORIGINAL COMMUNICATIONS.

I.

Three Cases of Gunshot Wounds of the Thorax, with Annotations.

By JOSEPH ARNOLD, M. D. Surgeon, H. M. S. Alcmene.

AS there are not many cases on record of gunshot wounds of the thorax, I transmit to you three, with accompanying annotations, which, if found worthy of admission into your Journal, are fully at your service.

The reports were generally made beside the patients, and I have not, by omitting anomalous symptoms, or removing incongruities, endeavoured to embellish the histories, at the expence of the real order of the symptoms as they daily occurred.

A reference to the figures will more accurately determine the situations of the wounds than an anatomical description.

CASE I.

ELIAS CAMERANOVICH, æt. 23.—June 28th.—Two days ago received a wound in the left side, from a leaden ball. It entered the middle between the nipple and top of the sternum, but did not pass out of the body. At present there is hæmoptysis, laborious breathing, anxiety, and increased pain on inspiration; countenance pale; pulse feeble and quick; belly slow; tongue white; thirst and loathing of food. Fiat extemplo sanguis e brachio ad

℥xxx. Sumat pilulas laxantes duas. Appl. vulneri cataplasma emolliens.

Evening.—Symptoms nearly as in the morning. Iterum mittatur sanguis ad ℥xxx; et quia alvus adhuc tarda est, repetantur pilulæ laxantes.

29th.—Inspiration is painful; breathing short, and accompanied with frequent cough; countenance anxious; the left shoulder is painful, probably from the ball being lodged there; pulse quicker, and more feeble; belly not yet opened; tongue white and dry. Capiat pilulas catharticas duas.

Evening.—Breathing, although somewhat difficult, is freer than in the morning; has had one dejection. Fluat iterum sanguis ad ℥xxx.

30th.—Last night a hardness was perceived, as if a leaden ball were lodged above the left scapula, towards its basis, and, upon making an incision, a round ball was extracted. He then took one grain of opium, and the pain ceased; at present his breathing is difficult, and he complains of pain of his chest; his countenance is anxious; bowels slow; urine (which is of a brown colour, and deposits a thick sediment) is voided frequently; pulse quick, and easily compressed; thirst; tongue white; heat of surface moderate. Sumat statim pilulas catharticas duas. Bibat ad libitum aquam cum succo limonis.

Evening.—Pulse quicker and weaker; breathing more laborious; anxiety and other symptoms aggravated. Fluat e vena brachiali sanguis ad ℥xlviii. Capiat opii granum unum.

July 1st.—Pulse feeble, and frequent: breathing slower, although still anxious and painful. A fetid bloody matter is beginning to discharge from the former wound. Rep. potus et cataplasma.

Evening.—Air enters the cavity of the pleura during inspiration; dyspnœa less urgent; pulse, as usual, is weaker. Sumat iterum opii granum unum.

2d.—Has had one alvine evacuation; stools mixed with blood In the morning his pulse was quicker and weaker, but towards mid-day it became a little stronger; in the afternoon a quantity of putrid lymph flowed from the anterior wound; tongue white; complains of thirst and frequent cough; expectoration bloody and purulent. Repetatur opium. Fructibus horæis pro ratione utatur.

3d.—Epistaxis came on during the night, and black fetid matter was spit up by coughing. The anterior wound is open, and the air enters, and is expelled as often as respiration is performed. A putrid sanies is also discharged; pulse still quick; bowels are open. Rep. medicamenta solita.

Evening.—Is affected with frequent respiration; the anterior portion

portion of the diaphragm is painful; the ribs raised with difficulty during inspiration; pulse quick and irregular; tongue moist; thirst abated; belly open; takes some food. Sumat opium et alia, ut solet.

4th.—A quantity of putrid blood, to the extent of half a pound, has been discharged from the wound; pulse weak, belly irregular; thirst still continues. Rep. med. solita.

5th.—Pulse is weak and quick; breathing as formerly, anxious, short and frequent; only two ounces of fluid have been discharged from the wound; has taken a little animal food; bowels open; stools bloody. Rep. solita.

6th.—Putrid lymph is still discharged from the cavity of the chest, in a full stream, during expiration; pain of chest easier; belly regular; pulse calmer (only 90); appetite for food moderate. Cont. med.

7th.—The wound being still open, a stream of sanies continues to flow; pulse regular; bowels natural; appetite improved. Rep. opium et alia.

8th.—Pulse regular; during inspiration air fills the cavity of the thorax, by entering by the wound; and 16 ounces of fetid purulent matter were discharged in a continued stream. Cont. remedia hesterna.

9th.—The integuments covering the former wound being removed, half a pound of putrid pus gushed out; pulse is natural; appetite improved; there is less anxiety in his countenance, and his breathing is not so short. Rep. med.

10th.—Pulse 90 and regular; appetite moderate; two pounds of putrid sanies have been discharged from the chest. Rep. med.

11th.—Only two ounces of pus have been discharged; other symptoms continue as formerly. Rep. med. quotidiana.

12th.—Cough, though not frequent, is still troublesome; pulse in the morning 90, but in the evening 120; two ounces of sanies have been discharged; appetite impaired; and his thirst is intense; belly natural. Capiat mist. salinæ effervescentis uncias duas tertia quaque hora. Pergat in usu opii.

13th.—A pound of sanies has been discharged from the chest, per saltum, upon coughing and expiration; pulse very quick; countenance anxious; respiration frequent and painful; distressed with cough; belly open; loathing of food, with thirst; has a desire for ripe fruits, such as apples, pears, oranges, melons, grapes, and figs. Rep. med.

14th.—A pound and a half of frothy sanies has been discharged from the thorax; cough still troublesome; appetite is returning; pulse 120; belly natural. Rep. solita.

15th.—Sanies less fetid but frothy is still discharging from the wound;

wound; pulse 100; belly open; appetite for food is moderate; and respiration is fuller than at any former period. Rep. med. solita.

16th.—Breathing less frequent; during sleep it is about 24 in the minute, his pulse being about 100; frequent cough; four ounces of pus, less fetid, but frothy, were discharged from the wound, upon the dressings being removed; bowels open; body much emaciated; appetite however is good. Rep. med.

17th.—He takes a considerable quantity of food; pulse 90; breathing free; cough troublesome; two ounces of frothy blood have been discharged; belly natural. Rep. opium et alia.

18th.—Pulse 100; cough frequent; belly regular; wound in the anterior part of the chest open; pus thicker and whiter; during respiration, air enters the cavity of the thorax by the wound, and is expelled from it again; emaciation and debility are increased. Rep. med. hesterna.

19th.—Emaciation is increased; a little pus unmixed with air flows from the mouth of the wound. Rep. cura et med. solita.

20th.—Pus is again copious and frothy; body emaciated; appetite increased. Rep. medicamenta.

21st.—Symptoms almost as yesterday; pus in small quantity; cough frequent. Rep. opium et alia.

22d.—Frothy and purulent matter is discharged from the anterior wound, and also from the wound on the scapula; pulse about 100, weak; copious discharge. Rep. remedia.

23d.—Copious discharge of frothy pus flowed from both wounds, which to appearance is thick and white; pulse frequent and weak; appetite moderate. Rep. med.

24th.—Frothy pus in great quantity is discharged from both wounds; pulse weak and frequent: appetite moderate. Rep. opium et alia.

25th.—Symptoms as yesterday; air enters the chest by both wounds, and escapes again mixed with pus; pulse varies, sometimes weaker, and sometimes stronger; appetite good; no pain. Rep. omnia.

26th.—As yesterday. Itirentur remedia.

27th.—Pulse varies; cough troublesome; wounds are open; anterior wound still admits the air; both discharge it mixed with sanies; body becomes more emaciated; appetite good; bowels open. Rep. med.

28th.—Pus discharged is whiter, thicker, and less putrid. Other symptoms as yesterday. Rep. med. solita.

At this period of the disease the patient left my charge.

Annotation.

The symptoms produced by musket-balls passing through the thorax

thorax and wounding the lungs, may be referred to several diseases mentioned in the nosology; and are such as designate pneumonia, empyema cum paracentese thoracis, emphysema, hæmoptysis, phthisis, hydrothorax, &c. and these symptoms sometimes appear progressively, but are oftener complicated in various degrees.

In the above case, I apprehend that they appeared in a more progressive order than is usually observed, there being an evident cessation of the inflammatory symptoms on the 30th of June, and a commencement of the marks of empyema, with its spontaneous paracentesis, on the following day; and I believe, that, by paying particular attention to these complications of symptoms, a strong indication will be presented to us as to the proper treatment of such cases.

In the history just related, the pneumonic stage prevailed for several days to so great a degree, as much to endanger the patient; and, on a reference to the daily reports, the very powerful means employed to avert the danger (which I conceive had the desired effect), will point out how necessary the most vigorous practice is, when such symptoms proceed to so great an extent.

The patient was of a robust habit of body, a prisoner of war, a native of Albania, and was wounded when employed in the boats of a Spanish corsair, near the Coronate Islands, in the Gulf of Venice. Two days had elapsed from the time of his being wounded before I saw him.

The ball entered midway between the left mamma and the sternal extremity of the clavicle of the same side, and, for the first two days, the direction that the ball had taken could not be discovered, till at length it was perceived as if passing through the left scapula, near its basis. The musket-ball was perfectly round, and not at all indented by having grazed the bones.

The symptoms plainly shewed that the left lung was wounded, and the passage of the ball renders it a matter of wonder how vessels escaped being injured that must have proved suddenly fatal.

On turning to the case, it will appear, that the inflammatory symptoms prevailed till the 1st of July, four days from the infliction of the wound, and I deem that their fatality was prevented in a great degree by the profuse bleedings, particularly that the large venesection had recourse to on the 30th averted the danger, which at that time seemed to threaten instant death.

I have mentioned, that I believe the symptoms at this early period were inflammatory; but there may be another opinion on the subject, namely, that the cause of their appearance was, that

the

the cavity of the left side of the thorax was filled with blood extravasated, or air effused into it, from the wound of the lung; and that the readiest means of relieving them would have been to have opened the external wound, and given a discharge to the oppressive matter; and, as it was probable that this opening would soon spontaneously have taken place, that no further risk would have been run by manual operation than nature herself would ultimately have produced; and, finally, that the relief afforded by the large evacuations of blood did not depend upon their diminishing inflammation, but on their emptying the vessels, and thereby affording more space for the action of the oppressed right lung. Leaving, however, these jarring opinions to the consideration of others, I may premise, that, on a perusal of the third case, here recited, (in which I suppose the lungs were also wounded, but where there was no stage of empyema, the wound closing without any dangerous symptoms,) this freedom of opening the cavity of the thorax without immediate and the strongest marks of its necessity, will be found very controvertible: perhaps it should never be done unless there be positive marks of one side of the thorax being filled with blood, pus, or air, exemplified by an evident enlargement of that side, by a pointing of the included fluid itself, by fluctuation being plainly perceptible, or pressure of the mediastinum on the opposite lung.

With respect to the particular state of the dyspnœa in these cases, I may mention, that the respirations are very quick and short; the patient seems to strive violently to respire by the sound lung only; and as the action of the ribs occasions great pain, he endeavours to perform this by the diaphragm and abdominal muscles, which are thrown into excessive panting action, which, by long continuance, produces great pain in these parts also, particularly about the anterior insertion of the diaphragm, to which this patient often referred more pain than to the wound itself.

The discharge from the wound in this case, and in most others that I have seen, was in very large quantity. For the first day or two, it was not very offensive, and seemed to consist principally of the serum of blood, which had previously been effused into the cavity. Expecting, therefore, that there was a coagulum remaining, I concluded that it would resolve, putrify, and escape by future evacuations. Accordingly the discharge, on the third or fourth day after the opening of the wound, became very offensive, and mixed with matter like the grounds of coffee, which I believe was the resolved coagulum. The smell of this discharge was the most dreadful that can be conceived,—so powerful as to overcome the by-standers, and to render the whole habitation

tion so noisome, that most persons were obliged to escape into the open air to avoid its influence.

The duration of this putridity varies according to circumstances; in some instances it does not cease for several weeks, which depends upon sloughs coming away till that time, by solution in the matter, and which are a source of putrefaction till the dead parts are wholly separated from the living, and the wound cleansed, after which time the offensive, aqueous, dark-coloured fluid, is changed to a mild purulent discharge, which, by diminishing daily in quantity, at length ceases altogether.

In some cases, especially when the discharge becomes thick, it is more or less filled with air bubbles, which sometimes are derived only from the air entering at the wound, mixing with the pus on its exit. I had but little doubt, however, in the case of Cameranovich, from its remarkable appearance, that it was effused into the cavity from the wounded lung, and mixed with the matter.

In none of the cases herewith described, did emphysema take place; perhaps it does not so often occur from gunshot wounds, as from those made by sharp penetrating instruments. I expected it anxiously in the above case, on account of the extent of the posterior wound; for the ball seemed to have passed out of the thorax near the spine, and proceeded under the integuments nearly as far as the middle of the scapula, which it had not passed through, as was at first apprehended, and, from this long extended wound, daily were discharged immense quantities of air and pus, though, in consequence of its valvular form, no air had ingress by it, unless when a probe was introduced to ascertain the state of the subjacent ribs.

I have seldom found any difficulty in rendering the escape of the inclosed matter free, without the aid of syringes, canulas, elastic bottles, &c. Indeed, in two of the cases, the bed-clothes were completely drenched with matter flowing from the wounds in torrents, as soon as the coverings were removed. The patients also, (I suppose from the relief they experienced from the discharge,) spontaneously placed themselves in such a situation as to render the orifice depending; and by coughing, expiring, and contracting the diaphragm and abdominal muscles, the matter gushed from their wounds like water from a tea-kettle.

I did not, in either of these cases, employ (what I believe in many may be found good practice,) injections into the cavity of the thorax, for the purpose of washing out the remains of the putrid matter, as well as any extraneous substances carried into the cavity by the ball. In the case of Cameranovich, it appeared that the ball entered the body without passing through his clothes,

so that the indication for injections could only be to wash out the remaining putrid matter, or perhaps correct its offensiveness by being impregnated with some antiseptic substance. In the following history of a similar wound, however, I have mentioned that an injection, even of a very powerful fluid, was employed for a great length of time, without detriment, and perhaps with advantage, as the patient, from a very perilous state, recovered under its use.

The next symptom which occasions the greatest distress in the wounds of the thorax is coughing, and this is more or less violent during the whole course of the complaint. For the first few days it is generally very painful, and (as in the case of Cameranovich) may be considered as an inflammatory symptom, or at least as very much increased by inflammation of the parts, and will therefore be relieved by bleeding and laxatives. In the more protracted state of disease, however, as it is occasioned by irritation from matter rising in the bronchi, we must look for relief only from opium, and such other remedies as appear necessary for the cure of the wound. In the above case, it will be found that, some days after the reception of the wound, the expectoration was bloody, and, at one period, was mixed with clots of black coagulum, which, I suppose, were removed from those branches of the bronchi which they had previously filled, by the lung collapsing about that time, and forcing them toward the trachea. About this time, also, epistaxis occurred, to the extent of about four ounces; an unaccountable symptom. The expectorated matter varied much during the whole affection, sometimes being purely muculent, at other times bloody, but oftener purulent, and consisting of matter similar to that which flowed from the wounds.

The cough in these cases, although sometimes continual, very painful, and distressing, is not of that violent kind which we often see after pneumonia, in which both lungs mutually assist violently to remove the offensive matter from the trachea by powerful and convulsive expirations: it seems rather to consist of repeated efforts to exscreate something irritating the trachea, which from the small quantity of air employed in filling the sound lung, are very often repeated, and ineffectual to produce their purpose; and, when it is considered that the patient breathes by one lung only, that the muscles auxiliary to respiration are weakened by over exertion, and painful in their action, it will not appear strange that this symptom should produce the greatest distress; accordingly I have found that it has been necessary to attend very much to relieve these ineffectual struggles by repeated and large doses of opium.

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The pulse, in these affections, is uncommonly variable in its strength and velocity; and I have not been able to discover any positive indication from its examination. I believe that, on the first few days, the quickness and weakness of the pulsations arose from inflammation, as we observe in gastritis and enteritis, for they soon became fuller and slower after a few bleedings. In the after part of the complaint, when the patient was worn out with excessive discharges of matter, I could not at all depend upon feeling the artery; for without any perceptible change in the other symptoms, or in the sensations of the patient, the artery would sometimes beat 120, and sometimes only 80 strokes in a minute, with more or less regularity.

The state of the bowels, in the case of Cameranovich, was remarkably regular, but rather loose than otherwise. On the third day, from what cause I know not, the dejections were mixed with clotted blood; the urine also, for several days, deposited a coffee-coloured sediment. I rarely found it necessary to exhibit laxative medicines; but the patient took fruits of various kinds, which probably had the effect of promoting this discharge. His appetite for food, after the wounds became purulent, was tolerably good, and remained so during the whole time he continued under my charge; so that I did not find it necessary to present to him medicines of a tonic and bitter kind, particularly as he had an utter aversion to taking them.

Thus I have denoted the symptoms, and made observations on them, of a case of wounded lungs, in which the patient lived under my treatment about a month, from the time the wound was inflicted; at which period, as he was a stranger, friendless and ignorant of the English language, he was put in a situation, that, in case of his recovery, he might be enabled to reach his native country, which was not far distant.

Little remains now to be said but respecting the prognosis, which, I believe, by all must be considered as of the most unfavourable nature, from the very first inspection of the wound; indeed I believe that it is from experience alone that we can ever be induced to expect a *fortunate* result from a gunshot wound of the lungs. In the case which follows, in which the prognosis was, if possible, more unfavourable than in that of Cameranovich, there was a perfect recovery; and it was from this case, and some others, in which experience seemed to be at enmity with all reasonable expectation, that I was much induced to expect an equally happy termination when he left my protection, at which period, though he was excessively weak from the long continued and profuse discharge, yet there were many circumstances that portended well; his respiration was much easier than it had hither-

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to been; the pus that escaped from the wounds was much less in quantity, of a healthy appearance, and nearly void of putridity; his appetite was tolerably good; his bowels regular; his spirits exhilarated with the hope of reaching his friends; although much emaciated, he was originally of a strong habit of body, and his age was youthful. The conclusion, however, was mortal. I had an opportunity to see him on the 2d of August; he explained to me that the discharge from the wounds was greater than ever, equal to three pints twice a day; his appetite was tolerable, but his medical attendants debarred him from fruits, and he had but a small quantity of meat allowed him; his pulse was very quick and feeble, and he was lying on a thin mattrass on the floor of an open smoky apartment. I called at the place on the 29th of August, and was told that he had been dead a fortnight.

Case II.

Christofolo Rodomere, æt. 50,—May 24th.—The bones of the left thumb are fractured, and the integuments and muscles are so much bruised, that the thumb is almost separated from the hand. The left side, from the hip to the ribs, is much bruised, and is quite black from ecchymosis. A leaden ball entered the left side, near to the tenth rib, three fingers-breadth from the spine, and came out of the body about an inch below the nipple of the left breast, towards the sternum.

This happened on the night of the 22d May.

As soon as he received the wounds he fell, and spit up blood by coughing, but after this he became quiet; his pulse was moderate; his breathing free; and he did not complain of pain. He took three laxative pills. Cataplasms were applied to the thumb, and wound of the side, and the saturnine lotion to the anterior wound.

25th.—Pulse natural (only 80); breathing free; has had two alvine evacuations; appetite good; no thirst; tongue moist; has slept quietly for six hours during the night; slight cough and hæmoptysis still continue, but the wounds are free from all pain. Rep. cataplasmata et capiat pilulam laxantem.

26th.—He breathes quickly, but without pain; functions otherwise go on well; pulse also natural. Continuentur medicamenta.

27th.—Respiration free; cough less frequent; hæmoptysis gone; wounds not painful; pulse natural; appetite good; tongue moist; has slept well; no thirst. Rep. solita.

28th.—Symptoms as yesterday. Rep. med.

29th.—Convalescent. Rep. solita.

30th.—The anterior wound is painful, a full inspiration gives pain; the pulse, however, is natural; belly slow. Fluat exemplo

plo sanguis ad ℥xx. Capt. etiam pilulas laxantes tres. Pergat in usu cataplasmatum et aliorum.

31st.—He is feverish, and has passed a restless night; he complains of pain of chest, and contused part of abdomen; breathing short; appears dejected; pulse quick; belly slow; appetite impaired; some thirst; tongue white; countenance pale, and discovers anxiety. Iterum fluat sanguis ad ℥xx. Bibat ad voluntatem, aquam succo limonum gratam redditam. Sumat pilulas laxantes duas. Applicentur fotus abdomini ubi sunt ecchymoma et dolor; vulneribus aliis cataplasmata.

June 1st.—Complains of great pain in the left epicolic region, where the ecchymosis is situated; fever still continues; has slept ill; pulse quick; belly loose; breathing hurried, without, however, much pain of chest; appetite impaired; still thirsty. Rep. fotus et cataplasmata. Sumat tertiis horis mist. salin. efferv. ℥iii.

Evening.—Pulse quick; countenance pale; respiration frequent, but less painful; wound in the vicinity of the heart is open; and, upon the dressings being removed, nearly two pounds of watery lymph, tinged with blood, and slightly fetid, were discharged; and upon coughing, the fluid poured out in a continued stream.

2d.—Symptoms still severe; countenance pale; pulse weaker; respiration more difficult; increased prostration of strength; anxiety greater; spirits depressed; expresses a desire of a visit from the priest for his spiritual concerns.

Annotation.

Christofolo Rodomere is a Venetian, and was owner of a rich vessel bound from Venice to Bocca di Cattaro, that was attacked by the boats of one of his Majesty's frigates, and after a violent resistance, captured; but not before her crew, which consisted of about a dozen persons, had killed or wounded twenty-seven of the frigate's men.

On a reference to the case it will fully appear, that, if the prognosis could ever be unfavourable, in this it must have been completely so. The patient was in years, was labouring under the misfortune of losing his all, had seen his brother and other relatives killed beside him; and, if any thing could be added to increase the force of his broken spirits, independent of his wounds, he was a prisoner of war, removed from that assistance that his circumstances enabled him to procure in his own country, and was in the hands of his enemies, in whose charity alone he could hope for assistance.

The thumb of his right hand was shattered to pieces, so as to require immediate amputation. The whole left side of his body

was black with a violent contusion, and a musket-ball had entered his back near the seventh rib, four inches from the spine, which had passed out at the region of the heart.

In this instance it was expected that little could be done with the probability of being ultimately successful; it was fully expected that suddenly some great vessel, or even the heart itself, would pour out its contents (for it is inconceivable how they could have been avoided by the ball), and prove suddenly fatal. But not so; there was not a single unfavourable symptom, except hæmoptysis (which was slight) till the eighth day after the infliction of the wound; no mark of fever, no loss of appetite, very little uneasiness in his breathing, and he had moderate sleep. On the eighth day, however, slight febrile symptoms came on, with some quickness of the pulse, paleness of the face, thirst, loss of appetite, and disordered respiration.

In such a state of things there were two circumstances that might be attended to, in order to form a ratio symptomatum. An inward hæmorrhage might happen about that time, and might give rise to the symptoms, or they might be the effect of a slighter degree of inflammation, such as often is observed in aged persons, giving rise to an effusion of lymph into the thorax. I had both these circumstances in view at the time, which was the reason that I refrained from such profuse venesection as I should otherwise have had recourse to. Considering the symptoms as much allied to those of hydrothorax (which they entirely resembled) I expected that relief might be obtained from the escape of the included fluid by the external wound, which seemed to be about to open; and on the following day a large effusion of watery liquor was discharged, which continued to escape without much appearance of putrefaction, as often as the coverings were removed from the orifice. The man's strength however decreased; his face became more and more pale; his respirations quicker, with tendency to syncope; his pulse feeble and irregular; and his appetite bad; so that, supposing his end drawing nigh, he requested to be enabled to receive the offices of the church before his dissolution; and, as it did not accord with the Roman catholic faith to entrust the Host in a ship manned by protestants, he was sent on shore, a fit lodging being procured for him, and it was expected that he would live but a few days.

After this time, however, a material change for the better was unexpectedly observed; his mind was rendered calm by the duties of confession, and the sacrament; he was now placed among persons who knew his own language, found some friends there; and an Italian surgeon had the treatment of his wounds.

From this period I had no opportunity of witnessing the daily variation

variation of symptoms. The medical men of the place principally employ as medicines such vegetable productions as are found near it; but they applied to me from time to time for some laxative pills for their patient.

I did not see him till the 21st of June. He was then much enfeebled and emaciated; the discharge from the wound had much decreased in quantity, and was of a more purulent nature; his respiration was easier; he was capable of lying down nearly in the decumbent posture; his pulse was quick and feeble, and his appetite variable.

The Italian surgeon had been accustomed to inject three times a-day into the cavity of the thorax, by the wound near the breast, (which continued to be fully pervious) a strong decoction of the leaves and heads of the Plantago lanceolata in wine. I was told that at first the cavity would hold ten or twelve syringes full (the syringe held about ℥iss), but that the quantity was now reduced to four, and was expected to diminish by degrees till the whole cavity would fill up.

As this is a practice and a remedy little employed at present in England, I examined more particularly into it. The fluid to be injected was thus prepared. A quantity of the leaves, stalks, and flowers of the plant were put into a pot, and as much wine poured on as would a little more than cover them, it was then boiled about a quarter of an hour, and strained for use.

The rationale of the practice I suppose is, that, by the astringency of the liquor, it tends to contract the sides of the cavity, or to promote adhesion between the previously inflamed pleuræ; it may also tend to thicken the included fluid by the tannin of the liquor uniting with the gelatinous part of the effused matter, and thus act by an antiseptic principle. Whether the plant is really possessed of any specific virtue to heal ulcers I cannot pretend to say; it has, for many ages, been employed as an application to wounds. Even the Roman physicans were accustomed to use it as an astringent for these purposes; and more recently, when surgeons were led away with the idea of gunshot wounds being poisoned, it was a common remedy, and great dependency was paid to it as an antidote to this particular kind of poison. It is probable, that, with some intention of this kind, it was employed by the Italian surgeon in the present case; though I believe that he principally used this kind of injection for want of others that he could not procure, as he applied to me for some tincture of myrrh to mix with water for the same purpose, with which, however, I could not furnish him.

I saw the man again on the 4th of July, when the symptoms were nearly the same as on the 21st of June. The pulse feeble

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and quick, the appetite bad, the emaciation very great; exacerbations of hectic every evening; alternations of colloquative diarrhœa and costiveness, and shortness of breath. The discharge from the wound was still considerable, but the orifice very small. On removing the covering, by expiring and straining, a quantity of white thick pus, without any putrid smell, escaped, (which was not mixed with air,) to the amount of about two ounces, and they reported that this quantity was discharged every morning and evening. The injection of plantago lanceolata was still persisted in. I recommended him to take a few pills composed of cinchona, opium and muriate of iron, which the surgeon requested me to furnish him with.

On the 8th of July, when I again visited him, the symptoms were nearly the same. The quantity of matter had decreased to about half an ounce twice a-day, and was of a good consistence. The breathing, however, was become more oppressed; the appetite was bad; the emaciation still increasing; he had a severe attack of diarrhœa, and the hectic exacerbations continued to appear every evening. The surgeon, with the hope of promoting his appetite, was giving him a few drops of a strong tincture of Chironia centaureum in water, twice a-day.

I saw him again on the 12th of July. He was excessively feeble, his pulse very quick and weak, his respiration easier; he was able to lie in the recumbent posture, and affirmed that he was much better. The surgeon told me, that, on the preceding day, on removing the covering from the wound, a membranous substance presented itself with the matter, which he extracted, and soon after another piece of the same kind was discharged. They were about three inches long, half an inch broad, and very tough. From their appearance, I have little doubt but that they were portions of the membrane of the thorax, which, perhaps, had been preserved from dissolution and putrefaction by the injection of plantain.

After these had escaped, a quantity of thickish, but unequal pus, to the amount of at least two pints, which was strongly putrid, flowed from the opening.

The breathing was greatly relieved by this evacuation, but he was excessively exhausted. I ordered him to continue the tonic pills, with the addition of a few drops of the tincture of Artemisia absinthium in water, after each dose.

The last time I saw him was on the 22d of July. He had much recovered since the last excessive discharge from the breast; the wound was healed; the breathing easy; the appetite very good; his strength returning; he was able to walk about his apartment, and had no cough. I heard afterwards, that, at the

the end of a month from this time, he was well enough to proceed in a small open boat, and even to pull an oar, across the Adriatic, on his way to his native place.

Before entering upon the third case, (which, although resembling the two others in the appearance of the wounds, was yet essentially different from them in the symptoms which followed) it may be proper to make a few observations upon what may be found in books respecting wounds of the breast; premising, that there seems to be no part of surgery on which more fallacious rules of practice have been prescribed; which have descended to our own times; and may be found even in some books published in the nineteenth century.

The work of Mr John Bell on wounds, it might have been thought, would have prevented the republication of former errors on this subject, evidently arising from an ignorance of the pathology of these organs. In his philosophical work are described the means necessary to be employed to obviate the symptoms arising from wounds of these parts. From his intimate knowledge of their anatomy and physiology, he has, as far as our present knowledge allows, given a satisfactory *ratio symptomatum*; and, perhaps with not less benefit to future practitioners, he has fully pointed out the errors of former surgeons, and given directions for avoiding them. A reference to his work, therefore, will enable most young surgeons to understand what is necessary to be done in most cases that occur, and which require surgical aid. In many other works, even of modern date, which are unfortunately in the hands of most medical men, will be found endless error, and futile observation.

What contradictory observations have been made with respect to the entrance into, and egress of air from, the thorax, from a wound of the parietes? and what fallacious means have been recommended to extract air, blood, or matter from the cavity? One gentlemen recommends, for the purpose of discharging the air from the cavity, that the patient should make a very full *inspiration*, and that then the wound should be suddenly closed; forgetting that, as the lungs are passive, and collapsed by the cavity being pervious to the external air, that inspiration, by raising the ribs, and depressing the diaphragm, increases the size of the cavity, which is filled by the air rushing in at the wound. Others recommend that the air should be extracted by means of syringes, elastic bottles, cupping-glasses, &c. various precautions being given to prevent the future ingress of it into the cavity, by closing the wound suddenly with plaster; not considering that the collapse of the lungs is probably the only means of safety in wounds of these organs, by preventing hæmorrhagy from the

vessels,

vessels, keeping the part wounded in a state of inaction, and the sides of it in contact; and, indeed, supposing that an exhausting syringe were applied to the external wound, in order to extract the air from the cavity, there can be no doubt but that, as fast it was pumped out, it would enter in by the wound of the lungs, and thus keep an uniform fulness.

Some have written much on the comparative size of the wound, and the canal of the bronchi, as if the cavity of the thorax, would not be filled, and the lungs collapse, after a few respirations by the smallest wound. It has even been observed, that danger would arise only from *inflammation* of both cavities, from there being an opening into both; not considering that the lungs would instantly collapse, breathing cease, and the patient immediately die. Another author, after advising the operation of introducing a large canula into the wound to empty the chest of blood or matter, to be repeated every twelve hours, till the whole be evacuated, adds, "if a ball have passed through *both* sides, the danger and oppression will be very great, and both sides of the thorax must be freed of blood."

By referring to many modern works, there will also be found as much fallacy respecting the diagnostic marks of the lungs themselves being wounded, as relating to the symptoms and method of treating such accidents. Surely the symptom of hæmoptysis alone is sufficient for this purpose. But it is observed, "when the lung itself is wounded, the air rushes out through the wound on inspiration, and enters the cavity of the thorax on expiration;" than which observation, nothing I believe can be farther from the real truth, nothing more contradictory to what we might expect from our knowledge of the function of respiration, and the actions of the different parts auxiliary to it. I could almost venture to say that the author of it had never seen a case of wounded lungs, or, having seen one, had not profited by his observation; and I might even proceed further, and affirm, that he was induced to offer this diagnostic mark from the imperfect knowledge he had of the powers of the lungs, as efficient to respiration, supposing, with many others, that they are active in performing their functions, and not entirely passive,—being raised or expanded only by the action of the surrounding parts: with which erroneous principles of physiology, it is no wonder that such fallacious histories, methods of cure, diagnosis and prognosis, should be produced, as appear in the systematic work of the gentleman here animadverted on. Yet this observation has been thought worthy of being removed from the original work, and republished in another modern compilation, with this change of diction: "When the lungs themselves are wounded, the air

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is heard to issue from the external wound with a hissing noise on inspiration, and to rush in with a similar sound on expiration." I believe that the quantity of air escaping from the wound of the lung into the cavity of the thorax must be very small indeed on inspiration; and if the external wound be sufficiently large to admit enough of air to fill the cavity, in proportion to the increase of its size on inspiration, there will be none at all, especially as, in cases of this kind, the patient breathes very quickly, and as much as possible without the aid of the ribs, on account of the pain occasioned by this kind of action.

It may appear proper to point out one work, which, on account of its cheapness and portability, is in the hands of most young practitioners, in which may be found the errors before mentioned, respecting wounds of the lungs, and which I think will sufficiently exemplify the mischief that must occur by referring to such books as rules of practice. This little book is called the Surgeons' Vade Mecum, printed in 1809, without the compiler's name being prefixed. At page 18, treating of wounds of the chest, it is said, that the diagnostic symptom of a wound not having injured the lungs, is, that "on each inspiration the lung protrudes through the wounded part;" which is so manifestly untrue, that it deserves to be exposed, especially as the work is evidently that of a person who has been in the habit of compiling, without practical knowledge of the subject sufficient to detect the errors of those authors from whom he has been accustomed to collect his works. Such a book, on its first appearance, ought to be so severely handled, as to render its noxious import of no avail, which is a duty that belongs to the reviewers.

A book which pretends to be a Vade Mecum, a compendium of a compendium, ought to be a collection of sentences, so accurately considered, so true in their import, and uniform in their veracity, that not one should be capable of controversy, unless of itself a doubtful matter. The work in question, however, is a compendium of errors and truths, such as were offered to the editor in his perusal of other works, and may principally be found in the third and fourth volumes of another compendium, called the Edinburgh Practice of Physic, Surgery, and Midwifery.

Case III.

Thomas Linton, ætatis 33.—This man was wounded on the 22d May 1812. A leaden ball entered between the shoulders, one inch from the spine, and came out above the clavicle of the same side, (viz. the right.) Another ball entered the left side, about the seventh rib, three or four inches from the spine, and came out two inches above the left mamma, in which place the ball was seen lodged in the skin, and was extracted.

24th.—Breathing quick and painful; pulse quick; no hæmoptysis; belly costive; tongue foul, and some thirst. Fluat statim sanguis e brachio ad ℥xxx. Capiat pil. laxantes iii. App. cataplasmata vulneribus omnibus.

25th.—Difficulty of breathing less severe; cough excites pain; pulse moderate; two alvine evacuations. Rep. pil. laxant. duæ et cataplasmata.

26th.—Wounds are painful; a full inspiration gives pain; cough frequent; pulse almost natural; belly open. Itiretur venesectio ad sanguinis ℥xx. Rep. catap. et pilulæ.

27th.—Respiration easy; cough less frequent; expectoration not bloody; wounds painful; pulse calm. Rep. cataplasmata.

28th.—Wounds painful; breathing free; no stool; pulse natural. Rep. catapl.

29th.—As yesterday. Cont. med.

30th.—Wound of the breast discharges pus, and free from pain; respiration natural. Rep. solita.

31st.—Continues to improve. Rep cura.

June 1st.—From this date, till the tenth, symptoms were favourable; poultices were daily applied to the wounds.

11th.—The wounds on the right side are almost healed from the left anterior wound; a large piece of linen cloth, perhaps about one inch square, was extracted. Rep. catap.

12th.—Some degree of dyspnœa; belly slow; pulse calm. Capt. pil. laxant. iii. Rep. catap.

13th.—A slight degree of dyspnœa still remains; belly slow. Rep. pil. laxant. ii. et catap.

14th.—Respiration frequent; cough troublesome; pulse moderate; belly natural; a portion of woollen cloth was extracted from the wound in the breast to-day, after which a quantity of thick white matter was discharged. Rep. catap.

15th.—The wound in the right side is healed; the posterior wound also of the left side is almost healed; the anterior still discharges a little pus; breathing quite natural; pulse calm; appetite good; no thirst. Rep. catap. vulneri anteriori.

From the 16th of June to the 20th all the symptoms have improved. 20th.—The pectoral symptoms are mild; wounds quite healed; a sense of straitness remains, which is increased on a full inspiration.

ANNOTATION.

There is a remarkable difference in the symptoms between this and the two prior cases, though, from an inspection of the wound when first received, we should probably have expected that the like symptoms would follow.

Thomas Linton is 33 years of age, of a fair complexion, and spare

spare habit of body. He is one of the men belonging to the English frigate, who were wounded in the capture of the Venetian Trabucculo, of which Rodomere was owner.

The shot, which passed through him above the right clavicle, in a direction towards the spine, did not produce a dangerous wound,—that, however, which entered his left side, and passed through the thorax, shewed much probability of the heart being wounded; and it is very difficult to conceive *how* it could have avoided the lung, so that, without regarding the symptoms that prevailed twenty-four hours after the time, I believe that most persons would look upon the prognosis as very unfavourable.

The symptoms that followed, however, were very anomalous, and not proportionate to the apparent injury. Could a musket-ball, and two considerable pieces of cloth, be carried through the lungs, without there being hæmoptysis to certify their being wounded? The external wounds closed without admitting air into the cavity of the thorax; but, if the lungs were really wounded, how did it happen the blood and air did not get admittance from the cells and bronchi, and give rise to empyema?

The only symptoms that evinced that the respiratory organs had been injured, were, considerable pain on inspiration, with other signs of pneumonia, which doubtless arose from inflammation produced by the wounds, and were immediately relieved by venesection and a few laxatives, and in less than a month the external wounds were healed.

Perhaps the reason why air did not get admittance into the thoracic cavity by the external wounds, arose from the pleuræ uniting early after their infliction, thus causing adhesions,—which seems the more probable, as the man ever since has been troubled with a sense of stricture between the wounds. The case, however, although unsatisfactory with respect to its pathology, nevertheless exemplifies, that a musket-ball may sometimes pass through the left side of the thorax, in such a direction as to portend much danger, without being succeeded by pernicious symptoms.

I have thus terminated a few casual remarks on three cases of gunshot wounds of the left side of the thorax, each one differing from the other in its symptoms, and all, I believe, would be looked upon as of unfavourable prognosis. It may be worthy of remark also to observe, that they are not three cases which terminated favourably, selected from a number of fatal instances of similar wounds, but are cases that occurred nearly at the same time, and were under my inspection at the same period.

Another observation worthy of remark, is, that in none of the cases was there any active secondary hæmorrhagy, although surely,

surely, if any part of the body could be expected to pour out its blood on the removal of its slough, it would be the lungs. I have mentioned that there might be internal hæmorrhage in the two first cases, but, externally, I scarcely saw one drop of blood. This observation is consonant to that of Mr Charles Bell, with respect to the rarity of secondary hæmorrhage after gun-shot wounds. Of the 23 men belonging to the frigate, wounded on the 22d of May, not one had secondary hæmorrhage, although many of them, when first wounded, bled profusely, and it was very difficult to conceive, in some of them, how arteries of the first magnitude were not injured or divided. The three Venetians, wounded at the same time, had no secondary hæmorrhage, although one of them had thirteen wounds, and another seven. As far as my experience goes on this subject, I may mention, also, that of about an hundred wounded men that came under my inspection on the 16th of January 1809, and continued under it till the 27th of the same month, I recollect but one case that required the tourniquet, (which became necessary on the day after the wound was received,) though there was certainly ample time for the occurrence of secondary hæmorrhage.

Vallona, Nov. 20, 1812.

EXPLANATION OF THE FIGURES.

Anterior View.

No. 1. Entrance of the ball in Cameranovich's case.
2. Exit of the ball in Linton's case.
3. Exit of the ball in Rodomere's case.

Posterior View.

No. 1. The place from which the ball was extracted in Cameranovich's case.
2. Entrance of the ball in Linton's case.
3. Entrance of the ball in Rodomere's case.

II.

Case of Cynanche Laryngea, successfully treated. By JOHN ARMSTRONG, M. D. Sunderland.

THE cases of Dr David Pitcairn, and Sir John Macnamara Hays, so clearly reported and illustrated by Dr Baillie *, and

* See the third volume of the Transactions of a Society for promoting Medical and Chirurgical knowledge.

and the judicious observations of Dr Farr, published in the Medico-Chirurgical Transactions, have forcibly drawn the attention of the faculty to that peculiar form of cynanche which affects the laryngeal organs in particular. And, as the instances hitherto exhibited were attended with fatal consequences, it is presumed, that it may not be entirely uninstructive to record an example of this formidable malady, terminating favourably, under the prompt employment of antiphlogistic measures.

On the evening of the 25th of May 1813, ANN REED, aged ten years, after some indistinct febrile symptoms, began to complain of soreness and tightness of her throat, attended with thirst, headach, hoarse loud cough, difficulty of deglutition, and respiration. Supposing that the disorder proceeded from cold, her mother bathed her feet and legs in warm water, gave her some tepid gruel, and put her to bed. But she soon became very feverish, and, having passed an uncommonly restless night, was brought to the Sunderland Dispensary at nine o'clock on the ensuing morning. The face was then pale, the countenance anxious and alarmed, the voice scarcely audible, and the respiration extremely oppressed and laborious. The patient struggled for breath, and took, with a kind of convulsive effort, long deep inspirations; the muscles of the thorax, and particularly those of the fore-part of the neck, being thrown into extraordinary action. There was not the least pain in the chest; she referred all her uneasiness to the thyroid cartilage, to which she often anxiously put her fingers, and seemed to feel as if something were almost strangling her in that part. On inspecting the fauces, the uvula and arch of the soft palate were found inflamed, and, in some places, slightly ulcerated, but there was hardly any tumefaction about the tonsils. Some water was purposely given her to drink, which she swallowed, though with great pain and difficulty. The tongue was moist, but covered with a whitish slime, the belly bound, the pulse about 130 in the minute, small, sharp, and vibratory, the extremities seemingly rather below, and the central parts of the body above, the natural temperature.

Mr Tulloch, the house-surgeon of the Dispensary, drew 12 ounces of blood from a large orifice of the arm, which induced some degree of faintness. The patient was conveyed home almost immediately afterwards, and took a powder, composed of twelve grains of ipecacuanha, and half a grain of tartrite of antimony, which was ordered to be repeated every ten minutes, till vomiting supervened. The second dose, however, operated freely as an emetic. Without loss of time, a large blister was applied over the larynx and external throat, and six grains of calomel, with two ounces of a strong infusion of senna, containing

two

two drachms of sulphate of magnesia, directed to be taken every hour, until copious stools should be procured. Thin gruel was prescribed as an ordinary drink and diet, and every thing stimulating expressly prohibited.

The calomel and purgative draught were thrice repeated, when plentiful and frequent evacuations succeeded. And, though the symptoms were rendered less violent, by the active means which had been successively tried, yet Mr Tulloch, conceiving that the disease still wore a threatening aspect, requested me, about four o'clock in the afternoon, to visit the patient. The appearances were then certainly not so unpromising as in the morning; but, as she complained of a constant sense of stricture in the larynx, and as the respiration was still difficult, and the blood previously drawn of a very inflammatory character, I thought it necessary to recommend immediate phlebotomy, and another vein was accordingly opened. When about eight ounces of blood had flowed, syncope took place, and continued for some time. Upon recovering from it, however, she was evidently relieved, the respiration being comparatively free, the countenance less anxious, and the pulse softer and slower than before the operation. The tartrite of antimony was prescribed in doses of a quarter of a grain every second hour, and the strictly antiphlogistic regimen continued as before.

The patient passed a sleepless night, but, on the following morning, the respiration was nearly natural; and though there was an occasional hoarse loud cough, it was accompanied with free expectoration. She had sweated profusely during the night, passed several liquid stools, vomited twice, and still remained sick, from the influence of the antimony. The blister had caused a copious discharge of serum, the inflammatory appearance of the throat was fast subsiding, and the deglutition much less painful and difficult. The tongue was moist, but still covered with a white fur, the skin of a natural warmth, the pulse 100 in the minute, soft, and compressible. An eighth part of a grain of tartrite of antimony was ordered every third hour, and milk, substituted for gruel, as the common drink and diet.

Though the cough was rather troublesome, the patient might be considered convalescent from this period, and in a few days was restored to her usual health, but her voice continued hoarse and indistinct for some time afterwards. Perhaps it may not be superfluous to remark, that nothing resembling a layer of coaguable lymph was either ejected by vomiting, or coughed up during the course of the disease.

The leading symptoms in the case above detailed, correspond pretty accurately with those which have been deemed the chief indications

indications of cynanche laryngea, and indeed appear to identify it with that disease. From the anatomical investigations of Dr Farr, it would seem, that, in this peculiar species of cynanche, the mucous membrane, investing the epiglottis, the glottis, and the external and posterior surface of the larynx, is the principal seat of the inflammation, which also, as the example before us serves to shew, extends over the common membrane, covering the soft palate and adjacent parts. And it is unquestionably to this inflammation that the characteristic symptoms are to be referred, namely, uneasy and difficult deglutition, partial tumescence of the fauces, painful sensation in the larynx, uncommon laboriousness of respiration, and fever *.

The decisive measures pursued by Mr Tulloch, in the first instance, I am fully persuaded, most materially contributed to the recovery of this patient. Though she was a very spare, delicate girl, that gentleman, with equal boldness and discrimination, copiously bled, vomited, and purged her in a very short space of time, and, doubtless, this judicious treatment made a strong impression upon the disease, which, though not actually subdued, was considerably reduced in its force, when my opinion was first required. The second bleeding, which I recommended, seemingly much accelerated what the other remedies had so successfully begun, and the timely operation of the blister, together with the further evacuations from the antimony, completed the cure.

In all acute inflammations, seated in vital parts, it is of great consequence not to depend much upon secondary means, but to employ, from an early period, the most powerful antiphlogistic agents, successively or together, that their influence may be so exerted as to produce a complete change in the circulation, with the least possible loss of time. The superiority of this mode of treatment I have repeatedly witnessed, and it will be readily admitted, that the present case affords a striking illustration of its efficacy.

Wearmouth Walk, Sunderland,
April 3, 1814.

III.

* See Medico-Chirurgical Transactions, Vol. III. p. 324, 325.

III.

Case of Tic Douloureux, cured by the external use of Tar. By Mr E. Colville, Surgeon, Ayton.

The following case occurred to me lately. I hope you will give it a place in your Journal. I can attach no merit to myself for the cure, as it was found out by chance; but I think it is of importance for medical men to know, that a severe disease, which baffles medicine in many cases, has been completely removed by such simple means, and I hope that future trials will prove it to be an efficacious remedy. Tar is in great repute among the lower classes, as a remedy for local pains and toothach, and I suppose is frequently useful. An instance of its stimulating effects was told me by a gentleman in this neighbourhood. He had been violently affected with toothach, and, by the advice of some person, he applied a little tar, inclosed in a piece of linen, to the hollow of a carious tooth. In the course of an hour his face became dreadfully swelled, and his tongue so much so, that it completely filled his mouth. A warm poultice was applied over all his face and mouth, and in two hours the swelling subsided, with relief from the pain. I have heard frequently of it as a cure for toothach, but never heard of such sudden and violent effects arising from its use in any other case.

Mr J. L. consulted me on the 8th of September last, on account of a severe pain in the left cheek and temple. It began about four years before that, without any apparent cause, and continued, with very little intermission. It seized him in paroxysms, which at last came on every three minutes. Whenever he attempted to eat or drink he was seized with dreadful pain; the tears gushed from his eyes, and he involuntarily pressed the part with both hands, with as great force as he could. He was deprived of his rest during the night; and in consequence he felt himself very feeble and frequently sick. His belly has been pretty regular, but when I saw him he was rather costive. He had been of very regular habits all his lifetime; his age about 72 years.

As his pulse was rather quick, I took eight ounces of blood from the left temporal artery, gave him some doses of sulphate of soda, to take one when necessary, and pills, each containing one-fourth of a grain of white oxide of arsenic and half a grain of extract of hyoscyamus, one of which he took evening and morning, and an embrocation of tinct. sapon. c. opio, to rub on the pained

pained parts, and drop into the ear. I saw him again three weeks after. The pain was not in the least degree relieved, except for a few minutes after he dropped the tincture into the ear. The sickness had also been in a great measure relieved, which he attributed to having taken the salts regularly. By his own desire I extracted three stumps from the lower jaw; and as the pills seemed to have had no effect, I ordered them to be discontinued. I ordered him to take a dose of salts every third day, and did not see him again until the end of October. His health was then pretty good, but the pain not in the least abated. I proposed the operation of dividing the nerve, which he would not agree to on account of his age.

I saw him again on the 9th of February this year, and was surprised to find him quite well. He told me that, in consequence of his daughter having found great relief from tar as an external application to rheumatic pains in several parts of the body, by my recommendation he was advised to try it for his disease, which he did about the beginning of January. He rubbed a little upon the cheek and temple for three nights successively. He felt no relief from it the first night; on the second the pain was rather easier; and on the morning after the third rubbing, the pain was quite gone, and he has never had the slightest paroxysm since, and is as well as he has ever been in all the course of his life. I think there can be no doubt that this was a case of Tic Douloureux. The pain indeed extended farther upon the temple than it generally does; but all the symptoms taken together make the case quite plain. It attacked in paroxysms, which were always severest when he attempted to eat; great nervous irritation; pulse a little quickened; face frequently flushed during the paroxysm; the muscles of the mouth frequently contracted, causing considerable distortion of the face on the left side, &c. I should be happy to hear if any gentleman meets such a favourable result from a trial of the remedy.

Ayton, 4th May 1814.

THE

THE INQUIRER, No. XXIV.

Remarks on the Advantages of Returning the Hernial Sac Unopened into the Abdomen.

"————*omnes*
Admonet, et magna testatur voce *per umbras*;
Discite *justitiam* moniti, et non temuere Divos*."
VIRGIL. Eneid. Lib. VI.

AFTER the very able treatises which of late years have appeared on the subject of hernia, it may probably appear a singular degree of hardihood to start any objections against the practice so universally followed, and sanctioned by so many great names. In fact, this particular malady may be said to have appropriated the talents of a greater number of eminent men than perhaps any other individual surgical subject. Gimbernat, Scarpa, and, in our own country, Mr Hey, Mr Lawrence, and Mr Burns, have successively investigated it; and, lastly, Mr Cooper's justly celebrated work may be characterized as a Herculean pillar—a *ne plus ultra* on the subject.

No one would undervalue the exertions of these illustrious surgeons, or on light grounds depart from their authority; yet this proper respect ought not to extend to a slavish adoption of their opinions, without examination and without reflection. It would be easy to prove that the authority of great names has sometimes exerted a debasing sway over the progress of knowledge. Few need to be reminded that a supine submission to the dogmas of Aristotle, and a superstitious veneration for his doctrines, merely *as his*, contributed to retard the advancement of science as much, perhaps, as the Cimmerian darkness of the gothic ages.

The several excellent observations on the subject of hernia, with which the world has lately been favoured, were surely never intended to set all inquiry at rest; on the contrary, it strikes me as being their chief praise, that they have stimulated the general body

* Perhaps the above motto, rendered literally, may be considered too strong and decided; but I wish it to receive a free translation like the following: "*From the shade of concealment* he addresses *all* in a distinct tone to this effect; estimate these observations by their *justness* only, and do not spurn sound arguments—the deities that ought to preside over and direct every conviction."

body of the profession to a more accurate anatomical knowledge of the parts concerned, and a more minute investigation of the whole subject than heretofore.

The proposal which I am about to disclose has long dwelt upon my mind, and been the subject of much reflection.

Several years ago, when I was an attendant on the medical lectures in the University of Edinburgh, ("Templa quam dilecta!") *Dr Monro senior* delivered from his place a variety of remarks on the propriety of returning the herniary sac into the abdomen unopened. I listened with the greater eagerness, as it happened that the observations of the professor exactly coincided with an unfledged idea to which my own mind had given birth, but to which I had never ventured to give utterance, being at that time entirely without the benefits of experience, and of more extended views. Even at this distance of time I can well recollect how forcibly I was affected by the reasonings, cogent in themselves, and enforced with all the eloquence and impressiveness of a professor, venerable for his years, and of the gravest authority from his talents, from his station, and from his multiplied opportunities of experience. I am anxious to embody the hints furnished by my note-book, brief and inadequate as they are, together with the result of my reflections since the period alluded to, and transmit them in this shape, seing no other likely way of the subject's coming before the profession.

The words of Hippocrates "ὁ καιρος ὀξυς, ἡ πειρα σφαλερη," may be justly repeated of hernia in a state of strangulation; the crisis is indeed most important, and the attempt at relief by operation, in the extreme hazardous. But here it fortunately happens that we are fully aware of the source of danger; for every surgeon is sensible that, in all instances where death takes place after the operation, it is owing to inflammation of the intestines, or of the peritonæum (the latter most frequently), having supervened to its performance. This applies to those cases where the operation is performed under the best auspices, and where it has been resorted to before gangrene takes place in the strangulated portion of intestine.

That inflammation should be so frequent and so fatal an occurrence, will not excite surprise, when respect is had to the condition of the parts immediately before, and during the operation. Probably for many hours the strangulated portion of intestine has been gorged with blood, and if not in a state of actual inflammation, is at least in a state that we may consider fully prepared, from the slightest additional causes, to take on that action. Besides, during this time, the parts have been farther irritated by handling, by pressure, and by local applications, now hot,

hot, now cold, to the tumour. Still it is irreducible, and we shall suppose the operation proceeded to without more delay.—What now takes place? The surgeon divides the integuments by a wound several inches in length, and then proceeds by an incision of similar magnitude to lay open the sac. After the viscera, thus unhallowedly exposed to the pernicious stimulus of a *medium* unusual to them (viz. the atmospheric air,) have been duly felt, fingered, turned over, and examined, *secundum artem*, the next step is to divide the stricture, which really one might suppose *a priori* to be the first object in view. The protruded bowels are at length reduced, but not until, in all probability, they have been brought into rude contract with a number of irritating substances, such as sponges applied to the wound, the fingers of the surgeon, or perhaps the sleeves of his coat !!

After all this, can it be wondered at if abdominal inflammation so often comes on and kills the patient? Do not experiments on brute animals prove the pernicious effects of air in exciting inflammation on abraded surfaces, and parts naturally secluded from its action? The sac being a portion of the peritonæum, must not the inflammation, resulting from so large an incision of the sac, rapidly creep upwards, and diffuse itself over the whole reduplications in the abdomen? Is not such wont to be the progress of inflammation in membranes? Considering the previous disorder of the *primæ viæ*, and in fact of the whole system, must not the farther violence done, during the operation, to a portion of the canal, be a most powerful cause of a fatal enteritis?

These questions admit, I think, of but one answer, and I cannot withhold my opinion, if in all cases violent inflammation does not take place after the operation, the happy circumstance is chiefly owing to the free detraction of blood previously practised as part of the *Taxis*.

Surely any surgeon would, at all times, be willing to reduce the hernia without an operation, if it is practicable, and will think it perfectly safe to do so. When therefore the operation is had recourse to in due time, what new reason makes a change of plan necessary? why will he open the sac *now*, when he would most willingly have reduced it but a few minutes before, by the means usually called *taxis*? Surely there is a manifest inconsistency in such a mode of procedure; and although a practitioner may screen himself from censure under the broad canopy of general usage, I suspect he will not be able, without sophistry, to elude the unsatisfied inquiries of his own conscience.

I again repeat, that the above observations proceed all along upon

upon the supposition that the operation has been performed in due time; for if the strangulation has been of such continuance as to render it more than probable that mortification of the protruded gut has taken place, then, beyond doubt, the sac must be opened for the obvious and intelligible purpose of removing the gangrenous portion, and affording the patient a chance of being saved.

The following, then, is my humble proposition: When called to a case of strangulated hernia, the first step to be taken is, to place the patient in a proper posture, to relax the femoral and abdominal muscles, and to apply judicious but *gentle* pressure to the tumour, at the same time that an assistant squeezes upon it from a sponge the very coldest water that can be procured.—Should this fail of success, let the patient be bled to syncope, and the same means assiduously renewed at the favourable moment of *collapse*. Should this too fail, a cathartic enema *(not of tobacco,)* should be forthwith injected. Purgatives by the mouth are ever pernicious, because, even were the state of the stomach such as to retain them, their only effect would be to aggravate the evil, by forcing down a quantity of *flatus* through the upper portion of intestine into the tumour.

Allowing the enema a reasonable time to operate, let the efforts at reduction be renewed; and should they still be unsuccessful, it may be fairly pronounced that the patient has no chance but from an operation; and therefore every minute that is allowed to pass in further temporizing and delay, carries away with it, beyond recall, a chance from the unfortunate sufferer. I think all the steps above recommended, might, by an active practitioner, be fairly put in force in *one hour at the utmost*. Should a trial so energetically conducted fail, the operation ought to be instantly resorted to, as farther attempts at reduction, in the present posture of things, is too often productive of much ensuing evil.

Let the integuments be divided in such a manner as freely to expose the upper part of the hernial sac; *without laying it open*, let the finger be gently insinuated along its neck: the fibres producing strangulation will now be felt, and may be divided in the usual manner. This being done, it is obvious that very gentle pressure applied to the sac will now be sufficient to return its contents into the abdomen.

The manifest advantage of this mode is, that a most formidable and often fatal operation will be converted into a simple incised wound of the abdominal parietes. The risk of subsequent visceral inflammation would be so comparatively small, as scarcely to be the subject of reasonable calculation.—"Utrum horum mavis, accipe."

The

The only objections of which I am aware are, the probability of the strangulation being occasioned by a *frœnum* passing across the mouth of the sac, or a quantity of fetid fluid being accumulated in the bottom of it. As for the latter, I think no harm can result from the whole contents of the sac being replaced in the abdomen, as the water would soon be absorbed. As for the former case, the cause of strangulation will be readily ascertained when the integuments are divided; and should it appear to be something in the mouth of the sac itself, a small opening may be made in its neck, just sufficient to admit of the obstacle being divided. But, I believe, the existence of a *frœnum* is not found more than *once in a hundred* cases; and for such a rare occurrence to be pitted against the operation here recommended, could prove no more than that *one* mode of practice is not *universally* applicable.

Surely enough has been said to render myself intelligible.

I trust the above observations will be entertained in that spirit of candour in which I have written them. Be this as it may, the terrors of impending criticism do not deter me from communicating these peculiar, though possibly erroneous, ideas. Freedom of thought is a prerogative inseparable from *mind*; I feel it to be so, and, within the limits of decorum, will assert it. "Dieu me l'a donnée; le diable ne me l'otera pas." My reason, such as it is, is only to be controlled by better reason, to which I am ever open, and ever most happy to listen. As for the above remarks, I am willing to flatter myself that their being anonymous will not detract from their weight, should they be allowed to possess any. Had I communicated any *facts*, or made any *assertions*, my *name* might very reasonably have been claimed as a guarantee for my veracity. But in what I have written, I have ventured no opinion without rendering a reason for it. I have not dealt in *assertions*, but in *arguments*, which may be judged of in themselves, and by themselves, independently of the person who urges them.

An arrow discharged from the archer's bow strikes with a force proportionate to the strength of the archer; so it is with *assertions* whose sole convincing power is derived from the name or fame of the *assertor*. But *arguments* are like arrows thrown from the cross-bow, which strike with equal force whether they are let off by the hand of a pigmy or of a giant; they derive their powers of conviction, not from the adventitious circumstances of the reasoner, but from the intrinsic strength of *their own* materials.

The sentiment is from *Bacon*, and I adopt it with all my heart.

February 1, 1814.

IV.

IV.

Fatal Case of Hydrothorax, with Appearances on Dissection. Communicated by Mr ARCHIBALD ROBERTSON, Surgeon of his Majesty's ship Cydnus.

HYDROTHORAX is, I believe, in all cases, a chronic disease; at least I do not recollect to have heard of any instance where it proved fatal, without having given previous indubitable signs of its existence, and palpable intimations of its progress and probable termination. In ordinary cases, too, a good deal of collateral evidence is to be deduced from the general state of the system. A leucophlegmatic and dropsical habit, a debilitated frame, a broken constitution, and a knowledge of the former habits of the patient, whether active or sedentary, are a sort of guide to the practitioner all along, and, in some measure, prepare his mind for the event, whatsoever it may be. But, in the case which I have taken up the pen to record, no such distinct notices were obtainable; the origin of the disease, and indeed its progress, for several days, were very obscure. When this circumstance is weighed, along with the extent of the morbid actions that dissection brought to light, one is struck with surprise and dejection, and is prompted to view it as one of those rare and extreme cases, which puzzle the simplicity of general rules, and disconcert all preconceived opinion. The subject of it was a man who had spent the greater part of his life in the active duties of a seaman, in almost every quarter of the globe,—a man certainly of an advanced age (for 54 may be considered so, especially in a sailor,) yet remarkably squat, muscular, hale and strong for his years;—a man who would have been the last suspected to fall a martyr to hydropic affections; yet in him "with all these appliances and means to boot," did the disease rapidly advance, and prove fatal on the *tenth* day from the very earliest feeling of indisposition!

He first made his appearance on the 17th of October 1813, complaining of an unusual sensation about the cartilago ensiformis, which he distinctly told me was not acute pain. He had first felt it at four o'clock that morning, on getting up to keep his watch. His pulse, tongue, and skin, as also (by his own account) his appetite and bowels, were perfectly natural. Thus no symptom of general or local illness being perceptible, he was sent away, with an injunction to call again, if he should feel himself worse.

Let me be permitted to pause in the narration for the purpose of remarking on the difficult circumstances in which a naval surgeon sometimes finds himself; circumstances requiring both delicacy and discernment. It is of all things necessary that the first and less prominent tokens of disease should be noticed, but it no less behoves him to have regard to the discipline of the service, lest by undue facility he give countenance to the practice quaintly called *skulking*. I apprehend it is not unfrequently quite impossible to distinguish the latent and insidious approaches of a serious disease from those feigned ailments, frequent amongst many individuals on board ship, who are prone to deceits of this sort, and always ready to dress up even their *real* complaints in mystery and exaggeration. With these facts ever before him, the surgeon is often obliged to form a judgment from what *actually appears*, rather than from what is *alleged*, and to subject the assertions of the patient to the touchstone of *time*.

The person in question returned on the morning of the 20th October, and stated that the uneasiness in the lower part of the thorax had not left him, but that, on the contrary, it was now attended with slight cough and difficulty of respiration. His pulse and tongue were still natural; the temperature of his skin that of health. In fact, the only apparent symptom was cough, with some expectoration of a viscid tenacious pituita.

Under these circumstances, his bowels were opened, and kept free; diaphoretics also were prescribed, together with the emulsion usually employed in catarrhus benignus. By these he felt somewhat relieved.

No more prominent symptoms appeared till the 22d October. Heretofore his appetite had been good; his face expressive of health, and his tongue clean; but on that day the first was impaired, and the latter white; yet his pulse still preserved its natural tone and velocity. Difficulty of breathing and cough had increased, and the sensation at the scrobiculus cordis had become an oppressive sense of weight. From these particulars, I was led to suspect an incipient effusion of the serous sort, probably (as I then thought) into the bronchial tubes of the lungs: I therefore ordered the tinct. digitalis purp. in such quantity as the state of the stomach would admit; kept the bowels free; and soothed the cough by the same mixture as before.

His complaint seemed to be stationary till the 25th, when early in the morning he was seized with a severe fit of dyspnœa and coughing; his pulse was *now* quick, weak, and small; face pale and anxious, and he felt it impossible to lie in the horizontal posture. A drachm of æther nitros. administered at the time

time in a glass of peppermint-water] gave him instant relief; but the former symptoms became, from that time, more and more violent.

The disease having now assumed so decided an attitude, the tinct. digital. was increased, the spirit. ætheris nitros. repeated, when dyspnœa, more severe than ordinary, made it necessary; a large blister was applied to the sternum, and two grains of opium exhibited at night to procure sleep.

Neither at this nor at any time during the rapid progress of the disease was there any external anasarca, general or local: nor could fluctuation of fluid be felt in the chest, probably on account of the great firmness of the muscles, and the quantity of adipose substance accumulated over the ribs.

The above remedies were persevered in to the end. None gave him relief, excepting the draughts of æther, and cough-mixture; and the ease they procured him was very transient. He continued to become worse and worse: frequent retching was superinduced by the severity of the cough; his pulse was very quick and weak; debility gained ground rapidly; his stomach was so irritable that it rejected all medicines, and other ingesta but thin sago.

On the evening of the 27th of October, he was seized with a fit of dyspnœa; retching and cough more severe than any of the preceding, and expired suddenly during the paroxysm.

Appearances on Dissection.

On opening the body I found that very strong adhesions were formed on both sides of the chest, betwixt the pleura costalis and pleura pulmonalis.

Both sacs of the pleura were prodigiously distended with a greenish serous fluid. The quantity was not ascertained by measurement, but I cannot possibly be deceived when I say it was *one gallon and a half!* In dissections of a similar nature, I do not remember to have seen such a profusion of water.

The pericardium contained, as nearly as I can judge, about a *pint* of fluid, of the same appearance with that in the cavity of the thorax. The heart itself was of an enormous size, at least *three times larger* than usual! The blood-vessels distributed on its muscular structure were whitish, and indurated, as if in a state of incipient ossification.

The lungs contained no tubercles, but were in every respect healthy.

The stomach was distended with flatus, but no marks of disease were to be traced on its coats.

The liver and other viscera had their natural appearance.

The peculiar features of this case are, that, even granting the effusion to have commenced on the 18th October, (which I humbly think not very likely,) the appetite was not affected till the 22d, nor the pulse till the 25th; and that, during the whole progress of the disease, some of the characteristic symptoms of hydrothorax never appeared. Death took place before any considerable absorption or wasting of the body had taken place; for, on commencing the dissection, I was somewhat surprised to find, that I had to divide about the thickness of an inch of adipose and cellular substance, before reaching the fibres of the intercostal muscles. Perhaps it is not refining too much to account for this fact, otherwise than from the rapidity of the disease; I mean by referring it to that general loss of action in the whole absorbent system which appears to be the ultimate cause of all hydropic affections; thus viewed, it becomes a sort of inverse proof of the fixed and formidable disposition of the malady with which I had to contend.

Regarding the whole circumstances of the case together, and comparing them with the quantity of fluid, the latter must appear great beyond reasonable expectation: Quere, *Considering the universal atony which takes place at death, might not a proportion of the serous fluid found, have oozed from the extreme mouths of the exhalent arteries after that event?*

The adhesions from their firmness appeared of long standing, and probably had no share in producing the cough. The lungs being healthy, there is reason to suppose that the cough was a convulsive action of that organ, produced by the stimulus and pressure of the effused fluid.

The enlargement of the heart appears to me to have been a *lusus naturæ*, or at least a congenital affection; for its parietes were as thick as usual, and its muscular fibre had the florid appearance of health.

Disease of the coronary arteries is a very rare occurrence. I think the celebrated Morgagni, *De Caus. et Sed.* (I quote from memory, the work itself not being within my reach,) has remarked, that when these arteries are affected, the *valves* also of the heart will be found in a coexisting state of disease. Analogy of itself would lead us to conceive so. In the present case, the absence of palpitation, and the regularity in the systole and diastole of the pulse during the whole course of the disease, may be considered some proof that such organic affection did not exist.

I regret that I have to leave this curious point undetermined. In truth, owing to accidental causes, which it is foreign to the purpose to detail here, I was prevented from pursuing the minutiæ

nutiæ of this dissection as I could have wished, and from ascertaining the *exact* quantity of fluid by measurement, &c.

Cydnus, December 1813.

P. S. I am happy in being able to say that the person whose case (concussion of the brain) is detailed in the Edinburgh Medical and Surgical Journal, Vol. IX. page 402, continued to do well, and has never since, so far as I know, been subject to an hour's indisposition of any sort.

V.

Case of Scirrhus of the Rectum. By GEORGE NESSE HILL, Esq. Surgeon, Chester.

To the Editors of the Edinburgh Medical and Surgical Journal.

GENTLEMEN,—Human suffering is on some occasions so great and so prolonged, as to render the office of a medical attendant extremely painful, but more particularly is this the case, when this suffering arises from ambiguous disease, the true nature of which he cannot understand, consequently neither remove, or even scarcely alleviate. Such a distressing situation is comprehended in the subsequent narrative, the perusal of which may possible serve as a guide to some attentive practitioner, how to steer his way among similar harassing difficulties, and enable him to mitigate the agonies of a fellow-creature. Such are the motives for my troubling you at present, and I assure myself, they will be sufficient to induce you to add to former obligations, by inserting the painful history of a case which has occupied much of the time and and attention of, Gentlemen,

Your obliged friend,

Chester, Nov. 30, 1813. GEORGE NESSE HILL.

The Reverend Mr ——, from his youth, up to near 30 years of age, had enjoyed very good health, an occasional tendency to ophthalmic inflammation excepted. Being very fond of riding, he thought a small hernia in the left groin had been produced by some violent exertion attending this recreation. The bowel never descended very low, and was always readily returnable. For this misfortune, he wore a screw truss of his own contrivance, which

not

not sitting easy when walking, was never used but in riding, and then he tightened the screw, which was fixed for that purpose over the centre of the pad. Residing in the country, and fond of field-sports, he took much exercise, but lived very temperately; was always regular in his bowels, having a daily evacuation, but occasionally liable to slight hemorrhoids, which appeared and retreated without the use of remedies. About the year 1806, he had a severe ophthalmic attack, with excruciating pain in the head; the inflammation appeared to assume the scrofulous form, and cold applications were highly grateful. A cloth dipped in cold water being one day applied to the eyes and whole head, the pain was instantly relieved, the ophthalmia soon departing; but he now first felt peculiar uneasiness, with pains in the lower part of the bowels of an unusual description, with what were called rheumatic pains in various limbs; but these last went off, returning occasionally, though the intestinal change never did.

About the commencement of the year 1808, he had a smart attack of the ophthalmia, which, upon retreating, was followed by unusual langubrs, and increased intestinal uneasiness. He consulted a veteran surgeon of this city, who told him that he had nothing materially amiss with him, but he was growing *hippish*, and must live more freely, and take a little generous wine after dinner. He returned home, and put this plan into execution for some days; but one evening being seized with most violent pain in the abdomen, attended with considerable pyrexia, he sent for the old gentleman. Upon his arrival, he observed he had somewhat mistaken his case, and that the wine plan must be abandoned. He took a large bason of blood from the arm, put him in a warm bath, and upon a water gruel diet. Thus this attack was removed, and in a few days he returned to his former state.

About the latter end of this year, and the commencement of the year 1809, he first perceived a sort of *permanent* odd feeling in the lower part of the abdomen, accompanied with a more than common frequency of desire to evacuate his feces, and particularly some hours earlier in a morning than he had ever been accustomed to rise. Towards the middle of the day, and in the evening, these urgent desires and uneasy sensations declined, and before bed-time were almost, but never entirely gone. Becoming worse in the autumn, he consulted a physician of considerable standing and practice in the profession. This practitioner was of opinion, that some hepatic obstruction, with dyspeptic symptoms, was the cause of all the uneasiness, and directed calomel with mild purges, salines and bitters. This plan was pursued with much diligence for many months, but without any material benefit. The calomel was given so as moderately to affect

fect the mouth, under an idea that a latent syphilitic taint might be the source of some glandular obstruction, but the patient was always positive he had never contracted syphilis. All these remedies, with tonics, opiates, &c. were persevered in for near a year and half, yet availed not to allay the peculiar intestinal feeling, and early morning urgency to evacuate the feces; in fact, every means adopted soon appeared inefficient. The matter actually discharged, he soon discovered to be nothing equal in volume to the urgency and tenesmus with which its exit was accompanied. This mode of proceeding was accompanied by a feeling, that, if he did not make it a regular practice to take cathartics, his distress would be greatly augmented, so that, at length, no sooner had he taken a hearty meal, but he felt impatient of its residence in the digestive organs. Thus, the round of being compelled to rise three hours before the usual time, spending the morning in taking aperients, and having straining evacuations, and the evenings in a little more tranquillity, day after day passed on. The physician still deeming the hepatic organs in fault, recommended a journey to Cheltenham. Eager to adopt any plan calculated to effect a change from such a distressing state, he very unfortunately mounted his horse to ride upwards of 100 miles in the warm spring of 1810. On his arrival at his destination, he of course felt very ill, and was compelled to consult a medical gentleman there, who in a few days told him, he conceived his case not of a description likely to be benefited by the Cheltenham water. His experience and feelings corresponded with this information, for the intestinal action excited by the water or the salts was peculiarly irritating; hence he determined to return home, and, strange to say, on horseback. Upon having performed half the journey in much distress, he was obliged to alight and recline on some timber for several hours, then walk to the next town, where, upon resting all night, and drinking soda-water, he felt so much relieved from heat and pain about the verge of the anus, perinæum, and urethra, with augmented intestinal suffering, that he accomplished the journey next day. Apprehending that a few days rest would remove the additional uneasiness, he was much disappointed to find himself mistaken; for, after this journey, he never regained his former ease about the parts last mentioned, which, the bowels excepted, had previous to it been well. He had also more frequent urgent desire to evacuate feces, with considerable straining, and now first observed they were of an unusual form, being ribbon-shaped, with occasionally a spot of blood on their surface. Having occasion to go to Liverpool, he was advised to consult an eminent physician there, who upon hearing his description of what has been narrated, said he thought it had

all

all along been more a surgical than a medical case. Conformable to this opinion, the assistance of a very able surgeon was obtained. Upon examining the rectum, with intolerable pain to the patient, this gentleman pronounced the existence of a stricture in the bowel, of that description which could be benefited only by a diligent application of wax-candles. With much agony he first passed a small one in the end of July, or beginning of August 1810, and occasionally persevered in their use for some weeks, taking some pills formed of cicuta, opium, and calomel, directed by the physician, who enjoined simple diet, principally liquid, and rest. With these directions, he returned to his home in the neighbourhood of this city, pursuing the plan with more steadiness than could be expected, considering that scarcely any of the pelvic contents were ever free from pain.

His appetite was very keen; he had no dyspeptic symptoms; no fulness or pain in the hepatic region; nor in the abdominal, except a little on the left side the navel, in the tract of the colon, as it goes down to form the sigmoid flexure. When he indulged in the use of animal food, all his sufferings were aggravated upon the descent of the ingesta to the lower intestines, so that he rarely took solids, though his desire for them continued undiminished. When he did occasionally indulge in animal food, he endured unceasing distress, till he had experienced increased fecal discharge from some aperient. The pain about the neck of the bladder, perinæum, and anus, growing daily worse, with some difficulty in passing the wax-candle or bougies, he sent for a surgeon from Chester. On his arrival, he did not deem it necessary to examine the rectum, but from the tortuous shape of the bougies when withdrawn from the bowel, and the declared opinion of the Liverpool surgeon, he considered the case as one of common stricture. He advised the patient to come to lodgings in this city, that he might have daily attendance. This was complied with, but accomplished with difficulty, being now very feverish, and having very bad nights, from ceaseless irritation, pain, and numberless straining stools day and night. Opiates, salines, and the constant use of aperients, with emollient injections, were directed, not omitting the diligent use of bougies, which, from the anguish they created, became daily more intolerable, and could only be used at all when the patient was under the influence of opium. The practitioner now in attendance conceiving, as others had done, that some lurking syphilitic taint might be the cause of all the obstruction and suffering, directed a mercurial course, which affected the mouth considerably, and, the subject believed, greatly aggravated his sufferings, particularly by producing great feebleness, and tendency to flatulence, unknown before. As the use

use of opium was increased, the appetite proportionally diminished; but no opiates would suppress the tenesmus, or scarcely diminish the pain in the perinæum, which originated in the Cheltenham journey. At length, after a few weeks of increased pain, with heat and throbbing, an abscess burst in the rectum, discharging nearly half a pint of pus, with a little blood. He became easier; his surgeon congratulated him upon this event, as likely to terminate his sorrows. However, a very short period was sufficient to produce the painful conviction, that no such fortunate destiny awaited him, for his stools still came off with the same form and frequency, and no less straining or distress. His peculiar intestinal feelings on taking animal food, and impatience to be rid of it by purges, remained unabated. The heat, throbbing, and fulness in the perinæum were nearly gone; the pyrexia was lessened, and for a few days he was comparatively easy. Delusive calm! He began soon to experience odd sensations in the membranous part of the urethra, attended with spasmodic constrictions of that canal, easily felt on applying a finger to its lower part. These came on at uncertain times, were preceded by a more frequent desire to render urine than common, and were only to be endured by taking large doses of opium. One day, in the absence of his surgeon, an apprentice called to see him, and, observing his torture, ran and procured him the quack medicine called the black-drop, from a large dose of which he fancied he obtained more ease than from laudanum. Thus this medicine became a favourite, and continued so to the end of his life. After suffering much of this urethral distress, superadded to the intestinal anguish, which remained inflexibly the same, in defiance of bougies, and all means, for several weeks, he one night, immediately before he was seized with an unusual spasmodic attack, thought he perceived wind rushing along the urethra, and, upon looking at the termination of the canal, perceived a large particle of feces sticking there. He took it off on white paper, and showed it to his surgeon, who was convinced what he saw was feces, but, for the sake of his patient, affected to be incredulous as to its coming up the urinary canal. Unfortunately, an early opportunity occurred, when the unhappy man showed him another spot in the act of making its exit.

Whenever flatus or feces entered the urethra, his sensations were indescribably acute, and violent spasmodic action, with tenesmus, and intestinal pain, followed in dreadful succession, so that the black-drop and the bed-pan were in almost constant requisition.

January 11th, 1812.—I was first desired to visit this gentleman in the temporary absence of his surgeon. I found him in a

state of great irritation from the causes already mentioned. As soon as a large dose of opium had in some degree tranquillized him, my first care was directed to the examination of the rectum. There was no stricture whatever within the reach of my finger, no internal hemorrhoids, no tumefied glandular eminences, no apparent induration of the bowel, nor any enlargement of the prostate gland; nothing but a solitary tubercle, or old pile at the anus externally, could be discovered amiss with the rectum. The examination gave little pain. I desired him to pass a moderate sized bougie as slow as possible, and when he arrived at what he termed the first stricture, to stop, and I would take hold of the instrument. This he accomplished in a quarter of an hour. What he conceived to be the first stricture, and the passing of which gave little or no pain, appeared to me to be nothing but the natural form of the bowel as it rises towards the colon. He resumed the bougie, and after another quarter of an hour had passed away, he said it was now beyond the second stricture. This operation was painful, but the instrument was within less than two inches of being completely passed into the body. He remarked, that, although this business was towards its completion often very painful, yet it was trifling when compared with what he endured upon passing a bougie through the first stricture, *before* the bursting of the abscess.

I took hold of the instrument, and, on very gently withdrawing it, I thought I could perceive a something peculiar about the upper part of the bowel, where it ends in the sigmoid flexure. The bougie, when withdrawn, had assumed nearly a spiral shape; the first bend was large, with two smaller ones, the end straight for about an inch, imbued with feces, and small stains of bright blood.

A moderated use of the mildest aperients, with a constant use of soft injections, and the smoothest diluent food, were directed. The rectum would now hold with ease from eight to twelve ounces of fluid for a short time. The appearance of the feces as to colour was very good, ribbon-shaped, of the width of penny tape, and though small in quantity at a single evacuation, yet in 24 hours more was discharged than would have been in perfect health; yet he was never easy in mind, except he was taking aperients, or using injections, continually exclaiming, "I am loaded with feces at the bottom of my abdomen—if I have not stools I shall be dead." He was directed to pass a bougie but seldom, and always after an enema, and to withdraw it on feeling pain; to go into a warm bath or a hip bath daily, as he found himself able, and incline to be as much on his left side as possible, (but this he never could effect, always lying on his back); to sit on an electric

electric stool when capable, and have sparks drawn from the abdomen and pelvis. On returning to bed, these parts to be well rubbed with a strong opiate camphor liniment. To return to the country as soon as the weather would permit. These directions were nearly correctly observed for some months, without material change, excepting two events,—one a temporary diarrhœa, the evacuations being bulky, though soft, and nearly colourless; the other a single discharge of blood in the form of a long dark coagulum, about four ounces, which came away after passing a larger bougie than usual, to excite a fecal discharge, an event he was always extremely solicitous to effect.

Not seeing him now so often as when in this city, I soon perceived he emaciated, especially from the pelvis downwards. He found he could promote the discharge of flatus, and occasionally feces, by lying for a few moments on his left side; but lying in any other posture than the back always produced augmented suffering, particularly in the course of the descending colon, where he always said the source of the derangement would be discovered to exist.

Subtances swallowed in food, as a currant for example, passed the bowels in about 20 hours, as did a small fish bone, which had been unguardedly taken upon his longing for change of food. But he never indulged in this way without exciting feelings which produced pungent regret, and the penance of a double dose of laxative medicine. His appetite rarely failed, and sickness was almost unknown to him. I was desired one day to examine his urine. It contained numerous small fecal portions, and a heavy fetid urinous matter at the bottom of the vessel. When the patient had not a fecal evacuation to any amount, per anum, for some hours, there would be no fecal portions in the urine. But the matter already mentioned was rarely absent, and occasionally in enormous quantity. A mild opiate injection was directed to be thrown up the urethra, and a bougie or elastic catheter introduced for a few minutes daily; the former was used a few times, and the latter attempted, but neither were followed up with resolution. An opiate enema was also ordered, and, that, conceiving it beneficial, was continued occasionally. His diet was now principally the yolks of eggs mingled with weak tea, and broth with onions, which he thought assisted him in expelling flatus, a discharge which always yielded much relief.

To medicine (opium and aperients excepted) he had taken the most insuperable aversion, and towards the close of the summer, the latter, just before they began to operate, gave so much increase of pain, that he dreaded their use, yet believed his existence depended upon their action. When this dread existed so

powerfully

powerfully as to cause procrastination in taking the dose, the evacuations were soon diminished in quantity, but not in frequency. When in this situation, he would become very heavy, or lethargic, lying in a torpid stupor for many hours, and then omitting his opium; but his irritation, with all its accompaniments, soon returned with augmented wretchedness, and occasional violent hot sweats, which did not diminish any of his distress.

Being naturally irritable and impatient, and entertaining a high opinion of the veteran surgeon he had consulted in the onset of his sufferings, he now sent for him again. Upon his arrival, he examined the rectum with much care and patience, but could discover no stricture, yet had apprehensions that one was forming near the anus. He entertained a belief that the liver was affected; directed small doses of calomel with opium, a laxative mixture, and one to expel flatus, with the occasional introduction of a small bougie into the urethra. These means were not complied with but for a short time, effecting no change.

Thus three parts of the year was spent. At the latter end of it he thought proper to leave his residence, and, with some difficulty was taken to the neighbourhood of Wrexham. Before his departure, I was requested to consult on the case, with Mr Wilkinson, surgeon of that place, and then it was I first heard of the inguinal rupture; he said it always descended when he had a stool, but readily returned by pressure. We concurred in directing a soft easy truss to be worn constantly, to save him the trouble of always applying his hand, as well as to prevent any sudden dangerous descent. We directed a continuance of the means before used, and the substitution of a tallow candle for a bougie, to be passed up the rectum occasionally, with the resumption of the catheter up the urethra, and the omission of bougies in any way. His pulse had always been very good; his tongue white, with red edges; thirst always moderate; appetite for animal food keen, and difficult to restrain, but if he indulged it, by taking a little soft fish, always thought it increased his sufferings.

For a few days previous to his departure, he was free from flatulent or fecal discharge *per urethram*, consequently from spasm and accustomed torture. This suspension buoyed up hope, which proved fallacious, for immediately on his arrival at Wrexham everything returned to its former state.

Before his departure I examined the rectum, but could not discover any stricture, only an unusual fulness of the bowel, as far as I could reach, and particularly about the anus. The tubercle or pile before-mentioned formed a sort of flap, which I wished to remove

remove when I first saw it, but he would not consent. Upon elevating this, a small hole presented itself, which admitted a fine probe to pass perfectly easy, about two inches and a half up the side of the gut; the sinus was narrow, and discharged a little good looking pus.

December 2d.—I saw this unhappy gentleman again, after near three months absence. I found him greatly reduced in strength, and much emaciated, suffering new distress from severe inflammation of the left eye, with extreme pain on the corresponding side of the head, taking a great quantity of black-drop, and still highly irritable; bowels and urethra much as before. During my stay he had a small highly fetid green stool, with a little blood; he had similar evacuations during the presence of the ophthalmia; his pains and spasms of the urethra were diminished. The means adapted to the relief of scrofulous inflammation removed the affection in about ten days.

December 15th.—He returned to Chester on a bed in a coach, bearing the journey well, and having a peaceful night, but in a day's time was as bad as ever, particularly from flatus passing through the urethra, which it did with an almost incredible noise and violence, accompanied by nearly as much feces as passed the anus, giving a feeling to the parts like cold clay and water passing out; previous to one of these bursts, the whole canal is rigidly spasmodic, which, suddenly yielding, the event follows.

January 23d, 1813.—The gentleman who first believed he perceived a stricture of the rectum, a few weeks previous to the bursting of the abscess, was now desired to meet me in consultation. He came from Liverpool for that purpose. Upon a re-examination of the bowel, he could not perceive any thing like stricture, but supposed an enlargement and diseased state of its glands to be present. It was agreed to inject an ounce and a half of infus. flor. anth. cum extr. conii et opio two or three times daily, and continue diet, with everything else, as before. These injections were as much in bulk as the intestine would now patiently endure; they afforded ease when he was sufficiently tranquil to use them, but this was seldom the case.

February 4th.—Appetite diminished, occasional nausea, loathing his egg food; intestinal distention at the sigmoid flexure of the colon, which, after continuing for some hours, a movement takes place like a body rushing through a narrow crevice, then into the urethra, and through the anus. The urine contains so much feces and degenerated animal matter, as now to constitute nearly one half of the contents of the chamber-pot.

April 2d.—Has had severe hiccough four days, with short intermissions;

termissions; says it seems to come from the bottom of his bowels, where the suffering commences. Pulse quick; skin hot, with alternate chills and sweats; appetite uncertain; never easy without constant use of the bed-pan; fancies fat animal food, as fat bacon boiled tender, useful to him. But the whole digestive organs, the stomach excepted, are now as they have been from the first, impatient of the residence of animal matters, even the mildest fish or a little gravy. Eggs and vegetables alone are easy occupants of the intestines.

May 5th.—Is astonishingly harassed with flatus, most of which bursts through the urethra, with inconceivable anguish, scarcely suspended by the largest doses of opium. When little or no feces pass by the natural road, the urine is tolerably clear, but, even under these circumstances, tenesmus is not diminished until something seems to give way, the first indication of which change is the cessation of spasm, when flatus and feces follow before they are evacuated per anum.

The stools have now resumed their healthy hue and odour. There is no prolapsus ani, be the straining ever so severe.

June 24th—No aperients have been necessary for these last four months; a small enema, about an ounce, is as much as the bowel will retain, which seems sometimes to facilitate easier evacuations. After having had very small ones for about 48 or 60 hours, he says he is loaden in the parts between the navel and left side; a quantity is suddenly discharged in the manner described, and short relief follows. The feces are always soft, however long retained.

June 28th.—Was attacked with violent ophthalmia of the left eye. A blister was applied to the temple, and it had no sooner vesicated the part, but a most severe erysipelas commenced on the nose, quickly spreading over the whole face, which became so enormously swelled, as to prevent him taking any thing material between his lips for some days except his opiate: they resembled two purple bladders. He was delirious; had frequent stupor, and occasional severe hot sweats. His sufferings in the parts so often mentioned were diminished.

The erysipelas subsided in three weeks, leaving a heavy hollow cough, and spitting of dense greenish yellow mucus. The cough came in violent fits, yet never brought pain in the breast or sides. A small blister was applied, but did no good, nor did augmented doses of black-drop diminish this teasing complaint.

August 10th.—Cough and expectoration increased, especially after eating; emaciation great, with immense evacuations of feces and offensive mucous matter by the urethra, mixed with bloody stains and a milky fluid like chyle. The urine for a few hours

hours is clear, of a pale yellow colour, that is when the obstructed spot in the bowels retains the feces from descending till they are become such a mass as to occasion their usual bursting off. The perinæal, urethral, and intestinal agony now as great as ever. The abdomen has no perceptible swelling in any part, nor does pressure on the left side the navel, where he has always complained, give additional pain. Takes from 240 to 300 drops of black-drop in 24 hours; appetite trifling.

September 10th.—Emaciation almost extreme; cough urgent; stools tardily evacuated, yet sure to come off sooner or later, without any means being used to hasten them; eats but little, but can bear many things he could not formerly; strength ebbing fast; pulse quick and feeble; expectoration great.

October 21st.—Stools for the last ten days contained much of the chylous-like fluid and the urine a great quantity of the fetid mucous matter. He had hiccough like a child who has eaten too much. I was called hastily at 8 o'clock this morning, the persons near him thinking he was fainting. On my arrival I felt thankful to the Great Disposer of all things, that the tortured frame of my patient had, in the quietest manner, paid the great debt of nature.

Dissection.

October 23d.—Being in a very indifferent state of health, my respected friend Mr Titley very kindly offered his services on this very interesting occasion.

We found the body greatly emaciated, and the lower part of the abdomen become of a greenish aspect. In the presence of Dr Thackery, Mr O. Roberts, a pupil from the Infirmary, and a relative of the deceased, I opened the abdomen in the usual manner, when Mr T., with great patience and accuracy, proceeded to display the contents of this cavity, and then that of the pelvis. The parietes of the abdomen could not have been thinner. The omentum was devoid of fat, but could not be raised on account of a firm attachment very low down on the left side; it was cut across and the portions reflected above and below. The liver presented itself in as healthy a state and as perfect a size as possible; the gall-bladder free from calculi, and full of pale bile. The stomach, with both its orifices, was equally perfect and healthy. There was a slight stricture of the bowel a little below the duct. com. choled.; it did not reach quite round, or much diminish the cavity; but from this point, down to the sigmoid flexure of the colon, no unhealthy appearance of the intestines could be discovered. Having removed them, a very large spleen presented itself, very dense of texture, but in no other respects

specis differing from health. The pancreas of the common dimensions and sound appearance; the left kidney very large, and pappy, with a large ureter, the right very small and firm, with a small one. The mesenteric glands had no marks of derangement from health. The lower reflected portion of omentum was drawn into a ligamentary cord, and firmly attached to the upper part of the sigmoid flexure of the colon by a point which might be called its apex. Below this the bowel was secured between two ligatures and divided. The commencement of the sigmoid flexure was perfectly sound, but, as abruptly as possible, became completely scirrhous. Almost a circular ring of hardness embraced it as thick as a little finger, narrowing the calibre of the bowel to one fourth of its natural size. But, in order the more completely to examine the pelvic contents, I advised with my ingenious coadjutor to saw off the front of the pelvis and take all carefully out. Upon this being effected, and the intestine traced from the sigmoid flexure, the most enlarged part from disease was at the commencement of the rectum, where the patient was accustomed to consider the second stricture as existent. The scirrhosity descended hence to near the verge of the anus. The bowel was about four times its natural thickness, of a firm texture, and dark greenish aspect, having a great number of various sized cavities or cellulæ, some as large as would have admitted the end of a large bougie, others would have been filled by a pin's head,—they were but shallow.

There were a few nipple like eminences of a deep red colour, and a number of ligamentary bridles running in various directions. A small quantity of creamy feces lodged in the intestine; its bore admitted a little finger, but would not allow of being stretched. Two small fistulous sinuses ran for two and a half inches up the right side the gut; the whole bowel resembled boiled leather. The urethra from its commencement to the membranous part was healthy; at this spot an opening was formed between it and the rectum, which would admit a goose quill. Upon pressing the inside of the intestine, feces issued upon the surface of the urethra; there was much confusion of parts in this junction and aperture. This part of the urethra had an inflammatory appearance, which was traced to the neck of the bladder; but neither here nor in any portion of the canal, prostate gland, or cervix vesicæ, was there any evidence of the parts having taken on a scirrhous action. The bladder contained about two ounces of the fetid mucous matter it had been accustomed to discharge, and although its parietes did not feel thickened more than ordinary, yet its internal coat was much diseased; it had the appearance of having a thick coat of varnish laid on, of the colour of

port

port wine lees, with reddish small tubercular papulous like eminences here and there. The left ureter entered abruptly into the bladder, with an orifice which would admit the end of a little finger; the orifice of the right ureter could not be found. The intense purple or red colour became a little less as it approached the sphincter, where it disappeared: the fundus of the bladder was not extensively adherent to the rectum. On removing the pelvic contents, the last of the lumbar vertebræ was observed by Mr T. to project very unusually forward, so as to diminish materially the natural diameter of the cavity. This bone not only projected thus, but at its junction with the sacrum it formed a sharp spinous edge. * The intervertebral cartilages of the lumber vertebræ were very large, and softer than ordinary. I was anxious to have preserved the pelvic contents, but could not obtain permission.

VI.

Remarks on the Fever which occurred at Gibraltar in 1813. In a Letter from Joseph D. A. Gilpin, M. D. Inspector of Army Hospitals at Gibraltar, to Colin Chisholm, M. D. F. R. S. &c. &c.

My dear Sir,

I Have received your letter of the 11th November, and am happy, and not a little flattered that mine to you, on the subject of the contagious fever of 1793, has met your approbation. During the months of September, October, and November last, a disease in every respect similar to *that*, was prevalent in this garrison; and had we not thinned our population, by turning eight thousand persons out of it, it no doubt would have proved as destructive as it was here in 1804. I am the only medical man that escaped the contagion; and what was more extraordinary, not a member of my family, six in number, caught it; and I know not another family of which that can

* Being out with a hunting party a short time previous to the commencement of his illness, he suffered a severe fall, which injured the lower part of his back so much as to prevent him from walking upright or without assistance for a long period.

can be said. We all of us took four or five doses of bark every day, by way of keeping up the tone of the stomach. I do not pretend to say that we owed our safety to that, but I did the same at Martinico, and escaped infection there also. When the disease ceased, the lieutenant-governor called upon all the medical men to give in a statement of their opinions, as to the nature, cause, and treatment of it. I inclose you the slight sketch which I gave, together with a statement of the deaths, &c. and an authentic document sent me by a well-informed Spanish physician, who volunteered his services in the civil lazaretto, established without the walls of the garrison, and which clearly, in my opinion, proves that the fever was *imported*. In the statements of fifteen medical men, two only seemed to be undecided as to its being contagious; the rest were staunch contagionists. In the street where it first appeared, the houses on *each side* were speedily affected. I had, with others, many numerous families under my care, the members of which were almost always progressively attacked. I attended the governor, his lady, and three children, with servants in all, amounting to upwards of twenty; the servants indeed were sent, at least most of them, to the military lazaretto. I have had some correspondence with Sir James Fellows, the inspector at Cadiz. From his account, the disease there was highly contagious; and he mentions a curious circumstance,—that a body of people who were stationed in a marshy situation entirely escaped the infection. I cannot at this moment lay my hand upon his letter, or I would give you his own words. The disease at Malta is the true plague, with buboes, carbuncles, &c. By the last accounts from the governor, it is now confined to one village, which they have walled round, and have placed a cordon of troops. I should be glad to have Dr Haygarth's work; I have no doubt of its being a valuable one. *Your* book, my dear sir, we have in the library, and it has been pretty well turned over lately, I assure you. I give you an extract of a letter to Earl Bathurst, I am certain it will give you pleasure, although it is so flattering to my feelings. The packet is about sailing, and I have returns, &c. to transmit to the Board. This must be my apology for this very hasty scrawl. Should you have any other commands, lay them freely upon me, being most sincerely and faithfully yours,

Gibraltar, March 1814. Jos. D. A. Gilpin.

By publishing the extract from Governor Campbell's letter to Earl Bathurst, mentioned in the foregoing, I may perhaps commit a breach of confidence; but it is so honourable to the character

racter of my friend, and does so much justice to his merit in the faithful discharge of his most arduous duty on this very trying occasion, that I cannot resist the desire to make the reader acquainted with it.

"*Extract of a Letter from his Excellency Lieutenant-General Campbell, to the Right Honourable Earl Bathurst, dated Gibraltar, 25th December* 1813.

"In having the honour to make this communication, I feel it incumbent on me to notice particularly to your Lordship, the great professional abilities, zeal, and attention, manifested by Dr Gilpin, deputy-inspector of hospitals, as the head of the medical department. The able manner in which he has conducted the arduous duties of his situation, during one of the most trying periods that could have occurred, claims my warmest approbation; and for which I trust, together with his claims for long service in the West Indies, your Lordship will have the goodness to submit the Doctor's pretensions to promotion, to the favourable consideration of his Royal Highness the Prince Regent."

OFFICIAL STATEMENT.

"During the months of August and September last, I saw some cases of fever, which appeared to me to be the usual autumnal bilious remittent; but on visiting two men in City-Mill-lane, I found them labouring under a fever, apparently of a still more serious type; but as I saw them only a few hours previous to their death, I had not sufficient grounds on which I could found a decided opinion as to the nature of the disease.

"I was soon after called by the late Mr Pigoli, to see some of his patients; and in their cases I recognized the symptoms and appearances of a fever which has been denominated typhus icterodes, and of which I had seen numberless cases in the islands of Grenada, Martinique, and Guadaloupe, when physician to the forces under the late Lord Grey.

"In the month of October, I attended different families, the numerous members of which were attacked in succession, and with a fever of the same type. Many of the symptoms were similar to those which accompany bilious remittents, such as anxiety, listlessness, alternate chilly and hot fits, white tongue, yellow skin, nausea, vomiting, &c.; but those symptoms and appearances which I considered as characteristic of the disease above named, were the protruded red eye, and exquisite pain at the bottom of its orbit, and of the forehead, back, and limbs; and, as the disease advanced, the dilated pupil, excessive irritability of stomach, hæmorrhage from the mouth and nostrils, dark vomit-

ing, skin of a dingy yellow hue, unlike the bright yellow suffusion of the bilious remittent, and in many cases a fatal total suppression of urine. To this catalogue I may add, that insidious cessation of symptoms, which almost always occurs about 60 hours from the first attack, raising hopes in the patient and his attendants that are speedily quelled by an aggravated recurrence of every fatal symptom. This deceitful calm I have often witnessed in the general hospital at St Pierre's, in Martinique.

"In the general mode of treatment, the medical officers of the garrison are, I believe, agreed. Calomel given at first, in rather a full dose, and afterwards in smaller ones, at the distance of three or four hours, seems to be the measure pursued; and it has been observed, that if the bowels be thoroughly opened, before the third or fourth day, the disease frequently proceeds with few untoward symptoms, and generally does so if the remedy affects the mouth. In the West Indies we hailed that effect as a very favourable one. In many cases, however, whatever might have been the previous treatment, a succession of alarming symptoms occur, and few more so than the extreme irritability of the stomach. Various remedies have been recommended, with a view to calm its violent action; the application of a blister; a table-spoonful, given at short intervals, of an equal mixture of lime-water and milk; solid opium in doses of half a grain or more, every two or three hours; pills made of Cayenne pepper, brandy, &c.; but it has been observed, that a repetition of purgative injections, and the patient's refraining for some hours (should his strength admit of it), from swallowing medicine or food, has had a good effect.

"The authorities for bleeding, in incipient cases of this fever, with a view to prevent congestion and subsequent topical inflammation, are no doubt very respectable; and, in some cases, when the patient was vigorous and plethoric, the practice has been successful. But we are now, I presume, possessed of a remedy in the cold affusion of water, which more speedily produces a solution of fever, and checks the inordinate action of the heart and arteries, from the continuance of which arises the congestion and topical inflammation, so much to be dreaded. It is a mode, too, of removing fever, which possesses one great advantage over that of the lancet; for though it should not produce the complete effect intended, it certainly does not diminish the patient's strength, but leaves him in a state that may enable him to bear the operation of any other energetic remedy that may be thought necessary: on the other hand, should the loss of blood fail in its effects, we may easily conceive how ill the subject of it would be preparedto struggle against a disease so frequently

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rapid and fatal in its termination. I cannot say, however, that I saw many cases here, in which I would have recommended the cold affusion; neither the state of the pulse, nor the heat of the body were such, as, in my opinion, to warrant the application of it. I preferred, therefore, frequent *tepid* spongings with vinegar and water. Children, indeed, were much benefited by the cold affusion.

"That the disease in question is contagious, (or, more properly speaking, *infectious*, as actual contact does not appear to be necessary to its transmission), I entertain not the least doubt, though an opposite opinion has been held by some highly respectable characters. I have witnessed, both abroad and in this garrison, too many melancholy instances of the disease being communicable from one person to another. At Martinique, in the year 1793, we suffered dreadfully from the ravages of a fever, in every respect similar to that which lately appeared here; and, in numberless instances, its infectious nature was ascertained, by its attacking those who were in attendance on the sick; and it is a melancholy truth, that very few of the medical officers survived the pestilential duty in which they were employed. But as, in this statement, I am only called upon to give my *own* opinion, I do not think it necessary to combat further the opinions of others. I shall merely add, that innumerable circumstances have brought a degree of conviction to my mind, that is not to be shaken by any arguments or reasonings on the side of non-contagion, that I have hitherto heard of or read.

"In endeavouring to account for the production of the disease in this garrison, many difficulties, no doubt, arise, which I do not by any means pretend to solve; but merely to state some of those which, in my opinion, militate against the idea of the disease having originated in Gibraltar.

"I find a difficulty, for instance, in viewing it as an endemic proceeding from marsh miasmata, or other noxious exhalations, as a considerable number of persons entirely escaped the disease, although strictly confined to the only situations where I conceive these effluvia may be supposed to exist, the dock-yard and navy garden. I find, too, a difficulty in attributing it to the peculiar state of the atmosphere, at the period of its appearance; for during the number of years that have elapsed since we have held this rock, surely the state of the atmosphere, and supposed prevalence of marsh-miasmata, &c. must frequently have been as they were in the years 1804, 1810, and 1813; and I have not heard that a fever of a similar type to that which prevailed during those years is on record. Local circumstances may have varied, no doubt, and concurred in rendering the type of a

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reigning fever more malignant, such as a great increase of population, crowded dwellings, &c.; but, for some years past, I am informed, there has not been much variation on these heads. I am inclined, therefore, to believe, that this infectious fever was brought into the garrison by a person who died of it, some time after his arrival, it was said, from a place infected previous to his leaving it; and from affidavits on record, it appears that it was introduced in a similar way in 1804; and had it not been for the prompt and vigorous measures pursued, the year 1813 would have exhibited as melancholy a list of victims.

"Jos. D. A. Gilpin, D. L. H.

"*Gibraltar, December* 1813."

Authentic official document (respecting the importation of infection), by a Spanish physician.

"The following is the mode in which, according to the nicest investigation, the fever was introduced into the garrison of Gibraltar, in this present year, 1813.

"On the 11th of August, a town xebeque, called the Fortune, whereof was master or patron one Lodovico Bosano, *alias* Moita, arrived from Cadiz. One of the crew thereof being ill, was sent to the Catholic hospital, who died on the 19th of said month, with all the symptoms of the fever now prevalent, namely the yellow-fever.

"A Frenchman, native of Paris, died on the 3d of September same year, in Governor's street, near City-Mill-lane, with the same symptoms as the former man, and who had also come passenger on board the vessel before alluded to; and, from the 3d of September to the 11th of the same, nine persons died in the neighbourhood. On this day I observed, in the same district, two cases of the yellow-fever, which I instantly reported to Doctors Gilpin and Fraser, who consequently adopted the necessary precautions upon such alarming accidents.

"It is also asserted, that a gun-boat which came in from Cadiz, about the beginning of August, had some of her crew put on board the prison ship, San Juan, who soon took the infection:—this fact will be better elucidated by the testimony of Surgeon Williams, of said prison-ship.

John Cortes."

"General

"*General Statement of New Cases and Deaths of the Epidemic Disease prevalent in Gibraltar, from the 8th September to the 4th December* 1813.

Date.	New Cases. Lazaretto.	Town.	South.	Military Hosps.	Total.	Deaths. Lazaretto.	Town.	South.	Military Hosps.	Total.	Detail of military deaths.	
To Sept. 17	54	17	0	0	71	16	17	0	0	33	Officers,	25
24	110	0	0	211	321	26	20	5	28	79	Royal artillery,	65
Oct. 1	134	45	8	165	352	60	34	0	61	135	R. sap. and miners,	18
8	101	94	11	232	438	50	21	3	49	123	4th R. veterans,	47
15	104	198	27	261	590	48	20	2	69	139	7th R. veterans,	62
22	81	181	15	289	566	46	84	5	76	175	11th detachment,	17
29	34	63	3	158	258	26	34	0	57	117	26th regiment,	54
Nov. 5	9	14	0	95	118	3	3	0	25	31	37th do.	55
13	8	6	2	59	75	6	4	2	16	28	Foreign recruits,	21
20	5	5	3	27	40	2	3	0	9	14	Barrack department.	4
27	0	0	0	9	9	0	0	0	3	3	Soldiers wives,	43
Dec. 4	1	1	0	9	11	0	0	0	4	4	Children,	6
Total	641	624	69	1515	2849	283	204	17	397	901	Total	417

"Jos. D. A. GILPIN, D. I. H."

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The foregoing highly interesting and important official statements, certainly require no further authority to fix in our minds a conviction of their truth and accuracy. Had any such been necessary, my friend, Dr Macgrigor, inspector-general of the hospitals of the armies under the Marquis of Wellington, has furnished ample testimony, in the following extract from his letter to me, dated St Jean de Luz, 28th February 1814. "During the last autumn, I had various communications from Malta, Gibraltar, Cadiz, Lisbon, and St Andero, at each of which, as you will have heard, a destructive fever raged. Sir James Fellows, at Cadiz, and Mr Fraser, the surgeon-major at Gibraltar, are decided, that their fever has been highly contagious; and from their statements, it would appear to have been the same disease which you have described, and which I saw at Grenada. Though the Spanish authorities have placed our hospitals at St Andero under the quarantine regulation, it does not appear that the fever there has been in any instance communicated by contagion. They had there the yellow bilious remittent. Some cases of it appeared likewise in the hospitals at Lisbon, Vitoria, and Passages." Dr Macgrigor subjoins a most important fact, which, although unconnected immediately with the present subject, cannot be too generally known, as the health of our armies, especially whilst on service, must evidently depend very much on the adoption of the measure it relates to. Its very great importance must be my apology to my excellent friend, for thus making public what certainly was only intended for my own information. "This army," Dr Macgrigor continues, "continues to enjoy an extraordinary degree of health, and to suffer small mortality from wounds or from disease. I cannot help ascribing, as the principal cause of both, my having established, for the last two years, the *regimental hospitals*, and making every corps take care, not only of their own sick, but of their wounded; instead of hurrying all to the general hospitals, and accumulating large masses of disease in different quarters, and thus producing contagion not only among the diseased, but the wounded. Unless when the army is actually in motion against the enemy, I never allow any cases but chronic, of long standing, and invalided men, to be sent to the general hospitals. Until this last season, I myself had no idea to what an extent the system of regimental hospitals could be carried. Until within these few last days, every battalion, and every company of artillery had its own comfortable hospital, though most of the corps were not only within shot, but actually in contact with the enemy. In the hospital of the 11th regiment, I actually heard a band playing in Bayonne." As this measure was found so practicable in an army on so large and

and complicated a scale as that of the Marquis of Wellington, why is it not made universal in our military establishments? It has unceasingly been my opinion, that general hospitals are the great sources of mortality in an army, as well as sources of exorbitant expenditure. In regimental hospitals, health and economy are united;—in general hospitals, death and a destructive waste of money. My own experience, which has been tolerably extensive, fully justifies this.

To these very satisfying and convincing authorities, I shall beg leave to add a few extracts from a most valuable work lately published by Mr Johnson of the navy, on the influence of tropical climates on the European constitution. This volume contains most interesting and important information. All medical men intended for a tropical field of practice, whether public or private, should not proceed to their destination without possessing the book, and making themselves masters of the rules of practice it contains. There is not, in truth, a more faithful or a more judicious guide. It will divest them of those absurd prejudices which ignorance and arrogance have united to give authority to. It will store their minds with facts of the highest utility, and amply supply them with the surest means of crowning the assiduous exercise of their arduous and important duties with success.

The highly interesting journal, under the head "bilious fever;" the still more interesting narrative under that of "contagious fever on board the Russel and Sceptre;" as well as the section on "the endemic of Batavia," with the annexed cases; present the most satisfactory proofs of two most important propositions, viz. the existence and propagation of contagion in tropical fevers: and second, the certainty of relief from the bold and judicious administration of mercury, to produce and establish ptyalism in those fevers. With regard to the first, Mr Johnson (p. 164.) most justly observes, "in respect to the propagation of the fever in the Sceptre (on the Coromandel coast), Mr Cunningham, the surgeon, a decided anticontagionist, has related the above particulars himself; and while he is unwilling to allow, does not attempt to deny contagion. These simple facts do not accord with the following, I think hazardous, sentiment of Dr Bancroft, Essay, p. 701. "The high temperature between the tropics is so unfavourable, I need not say to the generation, but to the existence of febrile contagion, that even when it happens to be brought into that temperature, *it cannot subsist*, much less propagate itself." I therefore most cordially unite my suffrage to Mr Johnson's, when he adds, "let us beware,

ware, then, of imitating the infidel, who, by subverting what he is pleased to call *prejudices*,

> Robs us of that which not enriches him,
> And makes us poor indeed!"

The result of the investigation and mature deliberation by a board at Bombay, consisting of three most respectable and experienced physicians, Dr Moir and Dr Scott of the medical board of the Presidency, and Dr Sandwith of the general hospital, was, that the fever which prevailed epidemically on board his Majesty's ship Centurion, in Bombay harbour, in the month of March 1804, "appeared to be contagious." On this result Mr Johnson makes the following remarks in a note, (p. 135.) "It has never been known, says Dr Bancroft, as I am informed, that a single case of this fever (typhus) had occurred on either side of the Indian peninsula." (Essay on Yellow Fever, p. 510.) If this be the case, and if the respectable gentlemen above mentioned, who had the best means of ascertainment *on the spot*, did not give an erroneous judgment, it follows, that *other fevers* may, under certain circumstances, *become* contagious. I am at a loss to conceive how Dr Bancroft will extricate himself out of this difficulty, unless he sets about proving that those distinguished gentlemen were entirely *wrong* in their decision. That this fever had a single symptom of typhus, Dr Bancroft will not, I am sure, allow; and it will require some ingenuity to invalidate, in the minds of others, the testimony of its contagious nature above adduced. In the next section, an instance still more in point will be shewn on the Coromandel side of the peninsula. I may here observe, that Dr Bancroft quotes Dr Wade, to prove that marsh fevers in the East Indies are never contagious; but he takes no notice of the opposite opinion of Dr Balfour, who had equal experience, and delivered his sentiments from the head of the medical board at Calcutta. "*Let us support our hypothesis by balancing testimonies, rather than suppressing such as run counter to our views.*" It is to be earnestly hoped that the Doctor will avail himself of this salutary admonition.

The result of the observation of the very ingenious and accurate writer of the section on yellow-fever, with respect to contagion, certainly furnishes no argument against my own observations on the fever of 1793, 1794, and 1795. This gentleman says, "contagion, as a source of this fever, is entirely rejected by those professional men who have the greatest opportunity of information, *now (1807) resident in the West Indies.* No case occurred where the fever could be traced to a contagious source." He

He adds, what certainly is a fact, respecting this fever, the higher grade of the yellow remittent, " In the very first stage of this fever, it would probably be difficult to distinguish it from the other continued fevers of the country." p. 241. It must be evident to the unprejudiced observer, and will, I doubt not, be so to this writer, as well as to Mr Johnson, on a careful review and comparison of facts, and the testimony of contemporary writers and observers, that the fever here described was very different from that which prevailed so fatally in the West Indies and North America, in 1793, 1794, and 1795. If more were required, the highly important documents from Gibraltar, which these remarks now accompany, and the very candid and liberal observations and strictures of Mr Johnson himself, (p. 108, 143, &c.) would be sufficient.

The second proposition which this valuable volume incontestably establishes, will appear still more evidently in the few following extracts. The argument thus furnished against those physicians who have of late railed so unmercifully against the mercurial practice, is one which cannot be opposed on any rational ground; and this must more clearly appear from the circumstance, that, to use the words of Mr Johnson, " if these honest and plain narratives do not remove every shadow of doubt, in regard to the efficacy of mercury in tropical fevers, all human testimony is vain. These documents are more convincing than if they came from myself, for I might either be blinded by prejudice, or have some interest in distorting the truth. Neither of these can have operated here, &c." p. 197.

Mr Johnson, in treating of the endemic of Bengal, informs us, that, " immediately after the operation of the cathartic, *the mainspring of the cure* must be acted on. For this purpose, from five to ten grains of calomel, according to the urgency of the symptoms, combined with half a grain of opium, should be exhibited every four or six hours, till ptyalism is well raised; when, in 19 cases out of 20, (I might say 49 out of 50), there will be a remission of all the febrile symptoms, and safety secured. This is undoubtedly the *sine qua non*, in the medical treatment of this fever, as well as many other fevers in the east," p 46. In the section on bilious fever, p. 146, he says: " one thing, however, is certain, and a very important consideration it is; namely, that as *the mercurial treatment, unassisted*, was here entirely followed, and implicitly confided in, both on board and at the hospital, so it will require some sophistry in its enemies to explain away these stubborn proofs of its extraordinary power and success." On this occasion, on board the Centurion, " out of full 150 cases of this fever, (contagious bilious fever, which, it

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will be readily granted, was no very mild or tractable disease), none died, unless subsequently at the hospital, out of the 32 left behind." At p. 72, he gives the practice of Mr Niel, surgeon of the Sceptre, the crew of which suffered much from the endemic fever of Madagascar. "The next and only remedy, where bloodletting and purging did not check the disease at once in its infancy, is mercury to excite ptyalism. I say ptyalism, *for soreness of the mouth* will not secure the patient in this endemic. In many of the fatal terminations, the mouth was slightly affected; but we never were able to excite ptyalism. Wherever this last could be induced, a revolution, as it were, in the whole train of morbid symptoms, instantly succeeded, and a healthy train supplied their place. This revolution was most strikingly evinced in the functions of the bowels, by the evacuations becoming all at once copious and feculent; a circumstance which, previous to ptyalism, no purgative, even of the most drastic nature, could effect."

These extracts, I imagine, are sufficient to prove incontestably this very important rule of practice in tropical fevers, and overthrow all the absurd objections to it lately presented to the public. But, before I conclude, I feel myself imperiously called upon to animadvert on some most unjustifiable reflections employed by Dr Bancroft on my practice in the malignant pestilential fever. They are so connected with the subject of Mr Johnson's book, that a fitter place for introducing my answer cannot be found. At p. 76 to 86, Dr Bancroft assumes as a fact, that I stumbled on the mercurial treatment of the malignant pestilential fever; and in a manner, I hope peculiar to himself, asserts, that I administered mercury even so as to excite copious salivation, because I *supposed, erroneously*, that the liver was the most affected part, and *had heard* of two surgeons of the army who, some years before, gave calomel with success, in the yellow remittent fever, not indeed to salivate, but to produce evacuations by stool, &c." This designedly false statement, for I do not mean to mince the matter, of my motives and reasons for the practice I adopted in the treatment of the malignant pestilential fevers of 1793 and 1794, will appear evident to any one who takes the trouble to read the whole of sect. 3d. ch. 2d. and the whole of ch. 4th, particularly sect. 1st of the 2d part of my Essay. He will there find that I had extensive experience of the great utility of this remedy, as far back as 1786, and afterwards in 1790, papers setting forth which were published in the Edinburgh Medical Commentaries; that I had, had that been necessary, the authority of Rush and Wright, which, put in competition with Bancroft's prejudices, and arrogant but baseless assertions, di-

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vests the latter of all pretension to importance and truth; and that, although the action of calomel in the cases of the 45th regiment, stated by Dr Lindsay and Mr Denholme, both long resident in the West Indies, and most respectable in their moral and professional characters, was purgative, yet the result proved the safety with which that powerful remedy might be given in very dangerous states of fever. Dr B. has said, I had heard only of the success of Dr Lindsay and Mr Denholme. I have to inform him, that the paper drawn up by these gentlemen was read to the Medical Society of Grenada, when I had the honour of being president of it, in October 1788, and the paper itself is still in my possession. The ingenious reasoning of the Doctor is completely overthrown, by the "honest and plain narratives," adduced by Mr Johnson. Indeed, it is difficult to discover what Dr B. and such writers would have; the only demonstration the subject is capable of he rejects; and he employs, as a disproving argument, the very basis of moral and physical truth, the usual order of succession of cause and effect obvious to human apprehension. "There is no source of error more common or productive than that of supposing an event which closely follows another," (as in the case before us where recovery from "yellow-fever" has followed or accompanied salivation,) "to have been occasioned by it; and it may be doubted whether a great number of the advocates for mercurial salivation in the yellow-fever, have any other or better foundation for their conviction of its efficacy." I confess candidly I wish for no better, unless indeed it could be mathematically demonstrated, or kindly conveyed to me by some infallible supernatural intelligence. If Dr Bancroft, as he represents the result of it, p. 69, was so successful with his "simple plan of treatment," how can he reconcile it with the excessive mortality in the army, in the medical staff of which he was a physician? The assertion so ill corresponds with the fact stated in my Essay, Vol. 1. p. 451, as to render it, to use his own mode of expression, "ridiculous and absurd." The questions must for ever return, and no doubt for ever puzzle Dr Bancroft, Why did not you and your brethren of the West India medical staff of 1796 and 1797, stumble (another of the Doctor's phrases), on a more appropriate and more efficacious remedy? why did you permit nearly 13,000 valuable lives to be lost, if you knew any more certain means of saving them? You say you employed more certain means; if you possessed the *divinum munus* you arrogate to yourself, prove the assertion by the result. In the mean time, why are the respectable evidences of the efficacy of mercury I have adduced, overlooked? Have they too uttered falsehoods as you say I have done:

Nate cave, dum resque sinit, tua corrige vota.

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It is scarcely necessary to observe, that the inference Dr Bancroft wishes to draw, (p. 81), from the supposed result of the treatment of the malignant pestilential fever with mercury, is altogether groundless, because the premises are absolutely false. I forbear further remark, particularly on what relates to my own case; (p. 83,) it might lead me into the use of language it is my wish and my intention to avoid; into an intemperance the subject would render irresistible. I take my leave of Dr Bancroft, I trust for ever, with the fervent prayer,—from such may Almighty God save my suffering country!

Bristol, 19th April 1814.

P. S. Since writing the foregoing, I have read, with much interest, Dr Brooke Faulkner's observations on the plague, as it lately occurred in Malta. The perusal has excited regret and melancholy reflection. It was indeed, as the candid and learned writer observes, "a disheartening subject." It is, indeed, very much to be lamented that any unnecessary restrictions were laid upon "scientific investigation to explore the nature of so destructive a disease, on so fair opportunity of obtaining useful knowledge respecting it." Under such an absurd order of things, it was indeed "of very little consequence whether medical men were employed or not." It is evident, however, that much good might have been done, even although the feeling the pulse was prohibited, by the knowledge of the symptoms Dr Faulkner has detailed, and, by a comparison of them with the accurate and distinct picture of the plague as it occurred in the British army in Egypt, given to us by Dr Macgrigor, and with the result of the practice there adopted in the treatment of it. It is evident, for instance, that the state of the brain, the high delirium, the wild staring look, &c. clearly pointed out the propriety, perhaps the necessity of one copious bleeding at the onset of the disease; and indeed we are told, "that in a few cases of blood being taken from the arm, it threw up a buffy coat, *and the patients recovered*;" that in other cases, although "the blood taken away in the same manner was in a very dissolved state, yet they proceeded well." It is evident too, from the report of Dr Macgrigor, and of the medical gentlemen connected with him in Egypt, that, under the dreadful circumstances stated by Dr Faulkner, there should have been no hesitation in giving mercury to the length of full ptyalism; and, in confirmation of this remark, we are informed "that some cases were detailed to Dr Faulkner by the surgeon of the 3d garrison battalion, in which he thought he succeeded in arresting the progress of the plague, by the very early and diligent application of mercurial frictions, giving previously

viously not less than twelve grains of calomel, in robust habits, to clear the bowels." This gentleman seems to have availed himself of the experience of our army-physicians in Egypt, in the use of mercury; for Dr Macgrigor says, "On the whole, in mercury and the nitric acid, we appear to have excellent remedies for the plague; *but they must be very early and very liberally exhibited.*" Medical Sketches, p. 189. In looking over the cases subjoined to the statement, I find that of nine only three recovered, and that of these three, two evidently owed their safety to a fortunate mistake of the attendant, who, instead of using a mixture of ʒi. of camphor, and ℥iv. of oil of turpentine, as an application to the carbuncle, administered the whole *internally*. The excessive purging brought on by this unheard of dose saved them. All apprehension from the bold exhibition internally of oil of turpentine, must now be removed. (See Edinburgh Medical and Surgical Journal for April 1814.)

Should the plague ever again occur, the comparison of the results of practice in Egypt and Malta, must, it is presumable, tend to displace fear, to encourage hope, and to establish confidence; but there must be no absurd restrictions imposed by those in power; and there must be no still more absurd and destructive medical prejudices. *

VII.

Answer to the Observations of Dr CHISHOLM, *on some Remarks of Dr* BANCROFT. By E. N. BANCROFT, M. D.

I RECEIVED, some weeks ago, the 36th Number of your estimable Journal, and found in it, (after an interval of almost two years,) a communication from Dr Chisholm, entitled, "Observations on some remarks of Dr Bancroft, contained in the 7th Appendix to his Essay on the Disease called Yellow Fever." Being unable, however, to discover any thing in these observations, which, if properly examined and contrasted with my 7th Appendix, could, in the smallest degree, invalidate any part of it, my first determination was to leave them unnoticed.

* Dr Chisholm desires us to state, that with this paper he takes his leave of this controversy. We had also resolved, that, with the insertion of the present communications, it should cease, so far as we are concerned. Dr Chisholm's first paper was admitted, because we considered it as a reply to some observations of our own, (Vol. VIII. p. 540); and impartiality demanded that Dr Bancroft's answer should have a place.—*Edit.*

ticed. I have, however, since changed that determination, merely because some of your readers may not have it in their power to make such an examination, by not having access to my essay; and it is only for their sakes that I request you will favour this, my answer, with an early insertion.

The first part of Dr Chisholm's communication, which seems to me worthy of the smallest notice, is that in which he complains of having been "furiously and grossly calumniated" by a "shameless slanderer:" a designation obviously intended for me, though he has not even attempted to justify it, or his complaint, by any proof, instance, or quotation; and I can, therefore, only repel this charge, as regarding myself, by a positive denial, and by an appeal, which I confidently make, to those who either have read, or may hereafter read my 7th Appendix. It is true, indeed, that, in order to establish the truth on subjects of high importance to mankind, I was compelled to adduce certain well-authenticated statements of facts, by which Dr Chisholm's veracity was unavoidably impeached; but, in discharging this painful duty, I studiously avoided offensive expressions, as far as was compatible with an adequate communication of my own ideas, and a fair vindication of the truth, even though I considered Dr Chisholm as having forfeited all right to this kind of moderation, by his very injurious treatment of others*; and it is worthy of remark, that, in the pages where he thus complains of me, he has professed to "take blame to himself for intemperance in his writings, and for the use of offensive language" to his opponents, even whilst the terms which he was then applying to me, were such as clearly manifest, that his pretended *contrition* had neither corrected his style, nor assuaged his *ireful* propensities.

After this exordium, I proceed to the *most important matter* in dispute; *i. e.* the supposed *generation* of that which Dr Chisholm has denominated a *malignant pestilential fever* on board the Calypso, with its supposed communication from that ship to the Hankey; and its pretended *diffusion* by the latter at the island of Grenada; and, avoiding the perplexed irregular course of Dr Chisholm's observations, I shall advert to facts, in the natural and perspicuous order of their occurrence.

My 7th Appendix began with an account (taken chiefly from the African Memoranda of that very meritorious naval officer, the late Captain Beaver) of the equipment and departure from England, on the 12th of April 1792, of the Calypso and Hankey, (with a small sloop,) having on board 275 men, women and children,

* See page 766 of my volume.

children, to form a settlement on the island of Bulama, near the mouth of Rio Grande, on the west coast of Africa. These ships having separated in a storm, the Calypso arrived 12 days before the Hankey; and, anchoring on the 25th of May, at one extremity of Bulama, Mr Dalrymple, governor of the intended colony, landed with many of the colonists, who, after erecting tents, or small huts, wandered about the island until the 3d of June, when they were surprised by the Bijugas, a neighbouring warlike tribe, who killed five men, with one woman, and wounded four men; carrying off, besides, four women and three children: a disaster which produced so much alarm and discouragement, that the remaining colonists hastily reimbarked, and, on the 5th of June, the Calypso sailed for Bissao, a neighbouring and very unhealthy Portugueze settlement; but, meeting with the Hankey in the Bijuga channel, she was joined by the latter, and both ships, (with the sloop,) anchored at Bissao on the 8th of June; at which time, as Captain Beaver mentions, (p. 52,) "the Calypso had many persons on board ill of fever, though none had yet died of it." He mentions also, that "nothing was heard but mutual reproaches from the people of the Calypso," who believed that sufficient care for their comfort and accommodation had not been employed by the members of the council; and were moreover "tired out with the length of their voyage, and irritated by the loss of their friends, (in the recent attack from the natives,) and by the disappointment of their hopes." Unfortunately these discontents were soon communicated to the colonists on board the Hankey; for Captain Beaver states, that he had "left her on the 5th of June, a quiet, clean, healthy, and orderly ship;" but that, when he returned on the 7th, he found her "a noisy, dirty, disorderly ship; the colonists dissatisfied and dispirited." Whilst these ships were at Bissao, the fever appeared, as Captain Beaver observes, on board the Hankey; and he adds, that, on the 21st of June, it "continued in both ships," though but "one person had yet fallen a victim to it in the Calypso, and none in the Hankey."---(See p. 60.) On that day, both ships sailed from Bissao, and, on the 27th, anchored in a fine harbour at Bulama. Discontent, however, and depression of spirits, had increased so rapidly among the colonists, that 16 of them separated almost immediately; and of the remainder, 147, including the Governor and Lieutenant-Governor, sailed in the Calypso, on the 19th of July, for Sierra Leone, in their way to England. On that day, Captain Beaver remarks, (p. 80,) that "the fever had hitherto continued in both ships;" that three persons had died of it on board the Calypso, subsequently to her departure from Bissao; and that she had finally sailed "with many

many sick." He adds, that "the Hankey had buried three also; and had now two colonists and three of the ship's company labouring under that disorder."

When the Calypso had sailed, the remaining colonists elected Captain Beaver to be their governor, and began to build a block-house on the summit of a hill, "covered with a thick forest;" but, having no shelter from the rains, which, in that part of Africa, fall in torrents at that season, a sort of roof was erected over the Hankey's deck, to enable them to live on board. The working people were, however, landed on the island at day-light every morning, (which is the most noxious part of the 24 hours) and brought on board to breakfast and dine, as well as to sleep at night. In this manner, they continued to labour through the rainy season, (which lasted until the 15th of October,) and afterwards until the beginning of November; when the time for which the Hankey was chartered being expired, it became necessary to land the colonists with their stores, though the block-house was unfinished. This being done, that ship sailed on the 23d of November, with 16 of the colonists as passengers; viz. seven men, four women, and five children. At that time, 48 of the colonists had died, subsequently to the departure of the Calypso; viz. 23 of fevers, one of fever and flux, and 14 of *other* specified diseases, accidents, and hurts.

This faithful, though concise statement of the facts, which were connected with the occurrence of fever on board the Calypso and Hankey, previous to their final departure from Bulama, presents nothing beyond that which, after the experience of nearly three centuries, might be reasonably expected, in ships so circumstanced, during the rainy and sickly season, which had begun on the 4th of June; especially when so encumbered with colonists; of whom the majority, according to the description given of them by Captain Beaver, in his letter of the 22d of November 1792, (to the Bulama trustees,) were "drunken, lazy, dishonest, and impatient;" and had moreover become *discontented* and *dispirited*, than which few things are more productive of disease and death in hot climates. It pleased Dr Chisholm, however, *whilst grossly ignorant* respecting these transactions, to fancy, and boldly assert, that a "*nova pestis*, a *peculiar original* foreign pestilence, recently generated, and *utterly unknown before*, endued with a new and distinct character, possessing *new powers of devastation*, and capable of propagating itself by contagion *throughout the world*," had been introduced "by the *Hankey*, on the 19th of February 1793, into Grenada;" and also to accuse *this* ship of having *originally* generated this most horrible contagion. But, having afterwards discovered that the fever which

which prevailed on board the Hankey had occurred *antecedently* in the *Calypso*, he found it necessary, (in his letter to Dr Haygarth,) to transfer this accusation from the Hankey to the latter ship, and to suppose, that the fever in question had been communicated from the Calypso to the Hankey. He thought proper also, from the facts stated by Captain Beaver, to divest this fever in a considerable degree of its wonderful and terrible properties, by representing it as the offspring of *typhous* contagion, resulting from an accumulation of *putrid animal* matters, afterwards exasperated to pestilential malignity, by intertropical heat. *Now*, however, he *denies*, (as I had done in my essay,) this "supposed *generative* power in vapours or exhalations arising from *animal* matters, accumulated in a putrid state;" but, having omitted to *invent* or *assign* any other parent, or generative agent, he has left this equivocal production, (as St Paul describes Melchisedeck to have been, "without father, without mother, and without descent.") But, at the same time, he endeavours, with undiminished confidence, to maintain its real existence, by asserting, (as he does at p. 418,) that this supposed malignant pestilential fever "was really and truly the typhus of temperate and cold climates, assuming a monstrous, a *new**, and most destructive form, through the agency of a tropical climate:" an assertion which presents to my apprehension a *climax* of contradiction and absurdity, considering that Dr Chisholm had previously asserted, (in his Essay, Vol. I. p. 95,) that the state of the atmosphere between the tropics does not seem to admit the generation of a high degree of infection;" and also, (at p. 103,) that, when the Calypso and Hankey sailed, "and during the voyage *out*, the crews and settlers *were all healthy*;" and that "no suspicion whatever can be entertained of the existence of any latent infection among them;" and considering also, that I had,

* If it were allowable to expect a *rational* explanation from Dr Chisholm, I should desire one respecting the *novelty* which he constantly and pertinaciously ascribes to the fever in question. Does he suppose that vessels with persons ill of typhus, had never sailed from Great Britain to the coast of Africa, or any other place of a similar temperature; or, does he suppose that this temperature produced *new* and *peculiar* effects on board the Calypso alone? The former supposition would be repugnant to *many notorious* facts, and the latter to *common sense*. If the heat at Bulama, which was scarcely equal to the ordinary temperature of the West Indian islands, could aggravate typhous contagion on board the Calypso, similar aggravations would have previously occurred in many other vessels. But, having *never* occurred, and it being, on the contrary, well known, that typhus is not only never produced, but always mitigated, and soon destroyed by an intertropical temperature, who will believe that it could have produced such *monstrous* effects on board the Calypso?

had, in different parts of my volume, adduced numerous facts, which render even the *possible generation* of typhous contagion, by any means, or in any circumstances, highly improbable.

In regard to the supposed malignant *exasperation* of typhus, by the agency of a tropical climate, I need only observe, that I have (between pages 506 and 515 of my work,) adduced a variety of the most instructive facts, some of which were observed by myself, and others by Drs Lind, Hunter, Blair, Trotter, &c. which all concur to prove, beyond the possibility of any reasonable doubt, or contradiction, that a tropical climate *invariably* produces effects the *most opposite* to those which Dr Chisholm, with such self-contradiction, ascribes to it; and that it not only does not exasperate febrile contagion, but, on the contrary, does invariably mitigate, and very soon destroy it completely. It is indeed notorious, that when typhus has prevailed during winter and spring in Great Britain, it rarely, if ever, withstands even the moderate warmth of a common English summer.

These well known truths (which Dr Chisholm has not ventured to deny,) make it impossible, on any rational grounds, to believe either his explanation of the means by which he supposes febrile contagion to have been generated on board the Calypso, or his assertion that such contagion did exist.

But though Dr Chisholm has not even attempted to give a single instance in which typhus ever was *exasperated by heat*, (his fiction respecting the Calypso excepted,) he is not the less confident in his *renewed* assertions concerning the reality of such an unprecedented and monstrous occurrence in that ship; and he even seems to imagine that these, his *mere assertions*, will be admitted as proofs. He tells us at page 417 and 418, that by his "*close* and *discriminative* attention" at Grenada, to so *unexpected* a visitor as typhous infection, it was rendered evident that, notwithstanding the excessive and illiberal vehemence* with which Dr Bancroft asserts the contrary, "typhus does exist within the tropics." Here, by a singular artifice, Dr Chisholm endeavours to *prove* a supposed fact by his *own unwarrantable assumption of it*. But as I have never observed the smallest appearance of that "*close discriminative attention*" with which he compliments himself,

* This imputed *vehemence* was merely ridicule. I observed, in a note to p. 702 of my volume, that if, instead of pestilential fever, Dr Chisholm had stated heat and putrid animal matters to have *generated elephants* at Bulama, his statement would have appeared less improbable; these animals having been frequently seen there, which never happened to pestilential fever.

self, whilst, on the contrary, his writings afford many proofs of a propensity to form hasty and erroneous opinions, and to defend them afterwards with obstinacy, I cannot believe, in *opposition to all past experience*, that so *unexpected* a visitor as typhous infection ever did manifest itself at Grenada, until I discover much better foundation for doing so, than that of Dr Chisholm's " close discriminative attention."

At p. 99 of his letter to Haygarth, Dr Chisholm had eagerly availed himself of a passage in Captain Beaver's Memoranda, from which it seemed probable that the latter, in June 1792, when a fever first occurred on board the Hankey, had thought that it might have been communicated from the Calypso; and this (as I formerly mentioned,) he probably had been induced to think, because at that time it was believed, even by eminent physicians, that all fevers were contagious. I added, however, at p. 699 of my volume, that Captain Beaver did not long entertain this erroneous notion; " but was soon convinced, by personal observation, that contagion had no influence in propagating the fever in question, because he did not afterwards even hint at the operation or existence of any such cause, in those parts of his journal and official letters in which he endeavoured to account for the sickness and deaths of the colonists, and of the Hankey's crew; and because, when he had become governor of the projected settlement, he did not adopt, in the smallest degree, any of those measures for separating the sick from the well, which, under his first erroneous notion, he had supposed to be of high importance; and which, with his great humanity and vigilance, he certainly would not have neglected, if he had continued even to suspect the existence of febrile contagion. And to prove still more decisively Captain Beaver's utter disbelief of the existence of any pestilential or contagious fever on board the Hankey, and of its introduction into Grenada by that ship, as Dr Chisholm had reported, I referred to p. 192 of Captain Beaver's Memoranda, where, after mentioning this report, he adds, " that, in consequence of it, the Hankey was sent to Stangate Creek, to perform quarantine, and orders were given for sinking the ship and cargo," but that, " on examination, the *falsehood* and *malignity* of this report being *proved*, this order was confined to the *Bulama baggage* only." I referred also to p. 305 of the same Memoranda, where, after noticing the losses which the owners of the Hankey had sustained by the report just mentioned, Captain Beaver stigmatizes that report as one " *for which there was not the shadow of a foundation.*"

Unabashed, however, and unrestrained by these decisive and reprehending contradictions, Dr Chisholm, without taking the

smallest notice of them, has endeavoured, even in the communication which I now answer, to persuade his readers, by a manifest perversion * of the obvious meaning of Captain Beaver's words, that a contagious fever was communicated from the Calypso to the Hankey, and that Captain Beaver had not only believed, but had continued to believe this, ten or twelve years after his return to England; and that, in so believing, he had "adhered to the unbiassed conclusions of his own reason, notwithstanding that attempts were made to bias his mind to the adoption of different conclusions;" of which attempts he asserts, (without a particle of truth, as I believe,) that "there appear sufficient proofs, in the passages from the African Memoranda, quoted by Dr Bancroft." This gross misrepresentation seems to have been intended to supply

* This perversion may be found in pages 423 and 424 of the last volume of your Journal, where Dr Chisholm has introduced two quotations from Captain Beaver's Memoranda; and pretends, that, by the first of these, Captain Beaver has admitted that a fever prevailed on board the Hankey before she had reached Bissao, and consequently before she had been exposed to marsh miasms; and that it may be proved by the second, that the Calypso's fever began *before* her arrival and disaster at Bulama. These quotations, however, do not, in the smallest degree, warrant the inferences which Dr Chisholm pretends to draw from them: but, on the contrary, they accord with my former statements. In the passage first quoted, Captain Beaver states only that the Hankey was free from fever on the 7th of June, and "that many days after (the 7th,) did not elapse, before the fever made its appearance on board that ship also." But as both ships are stated to have arrived at Bissao, the very *next* day, *i. e.* the 8th of June, the "many days" in question could not have intervened until after the Hankey had been at least *some* days at Bissao, and therefore the passage is in *direct opposition* to Dr Chisholm's assertion. In the second of these passages, Captain Beaver, alluding to the slaughter, &c. of the colonists at Bulama, observes, that "its immediate operation, towards the failure of the enterprize, was the delay which it created, before we could commence our labours, *ere* which the fever had made some havoc." Now, this proves that the fever did not begin until after the Calypso had been at Bulama, where her colonists were slaughtered, &c. and that it had only made some progress before the commencement of the labours of the colonists; which labours did not commence until after the Calypso had left Bulama, on the 19th of July, to return to England; and consequently it demonstrates, that Dr Chisholm has grossly misrepresented the plain import and meaning of Captain Beaver's words. That he could have done this from a deficiency of intellect, I do not believe; and therefore I am compelled to infer, that he has done it with a hope that his readers, from a want of attention, might not detect his misrepresentation, and probably in this hope he pretends to *exult*, as if he had gained a victory: adding, with an air of triumph, that "it would be fatuity indeed, to harbour a doubt of infection having been received by the people on board the ship, (the Hankey,) on her rejoining the Calypso, and afterwards communicated to the unfortunate people who had intercourse with her at Grenada," &c. "That the reasoning of Dr Bancroft is mere sophistry, when thus tried by the touchstone of plain unsophisticated observation, and common sense," &c. My readers will form their own opinions of *this fallacious language*.

ply the place and *loss* of one still more gross and unpardonable, which I had exposed and confuted in my 7th appendix, and which Dr Chisholm had advanced at p. 102 of his letter to Haygarth, where he asserts the fever which had prevailed on board the Calypso and Hankey, and which had attacked Captain Beaver, "to have been *direfully contagious*;" and that, in one instance particularly, *three* persons who were near his (Captain Beaver's) bed during his own illness, from which he almost miraculously recovered, *received the infection from his person, and died soon after.*" To prove this confident, though groundless assertion, Dr Chisholm referred to pages 171 and 172 of Captain Beaver's memoranda, where indeed mention is made of his illness, and of *three* persons who died in the space of two days; *but without any fact, circumstance, or opinion, indicating, in the slightest degree, the existence of any febrile contagion*, or any, even the smallest, connection between Captain Beaver's disease, and the disorders of the persons who had so died. Two of these persons, viz. Mr and Mrs Freeman, are stated by Captain Beaver to have laboured under the fever which occasioned their deaths, *thirty four days before the commencement of the fever* under which Captain Beaver then laboured, and which, from the account given of it at pages 114, 121, 161, 173, 174, and 375, of his Memoranda, *was indisputably an intermittent*, and had recurred at *seven distinct periods*, with considerable intervals between them; consequently it must have been destitute of any contagious quality. In regard to the other of these persons, Mr Fielder, who appears to have died suddenly, no intimation is any where given of the cause of his death; and there is, therefore, no reason to conclude that it was fever, and much less fever derived from contagion.

After a minute examination of facts connected with the deaths under consideration, (at pages 705, et seq. of my 7th appendix,) I declared myself unable to discover any thing which could excuse, much less justify, Dr Chisholm for the serious liberty which he appeared to have taken with the truth, on this interesting subject, "in direct opposition to facts, printed, and stated so distinctly, as to afford no room for mistake;" and I added, "that unfortunately the impressions resulting from this *incident** would not be weakened by *others*, which must afterwards fall under our notice." This was an intimation which I thought sufficient to excite any man to attempt a vindication of his veracity, who did not

* I had here designated the invention of a positive untruth, tending to mislead medical men on a subject of high importance, by the *mild* and inadequate term of *incident*, because, I wished on this, as on other occasions, to avoid offensive expressions.

not believe the attempt hopeless. But neither in regard to this, or any other of Dr Chisholm's positive deviations from the truth, of which he was *distinctly* and *repeatedly accused* in my 7th Appendix, has he attempted to produce any justification, excuse, or explanation; though he has ventured upon *new perversions* and *misrepresentations*, such as those specified in the last of my notes.

It appears from Captain Beaver's Memoranda, that, after the departure of the Calypso, the remaining colonists were *laboriously* employed on shore during nine hours daily; and that (besides exposure to marsh effluvia) they were often caught in and assailed by torrents of rain, which, in that climate, are known to occasion dangerous fevers. These causes, joined to various hardships, and to a *general despondency*, produced a rapid increase of sickness, as might have been expected, among the colonists, who, as Captain Beaver states, (in his letter to the Trustees, of the 22d of November 1792) were "habituated to drunkenness, idleness, and all the vices of the capital;" so that, on the 17th of September (as is mentioned at p. 137 of the African Memoranda) "*every person employed about the Block-house* was ill, and unable to work." That this illness resulted from the causes just mentioned, and not from the influence of febrile contagion, may be fairly inferred from this, among other facts; viz. that while the people working *on shore* had *all* become sick, the sailors, who *lived on board* the Hankey, were comparatively healthy, and continued so until that ship left Bulama, on the 23d of November; at which time, as Captain Beaver attests, *not one of her crew had died*, though the deaths among the colonists amounted to more than half of their number. But if *direful* contagion had operated, as Dr Chisholm pretends, the consequences must have been very different; for the sick being kept and *nursed on board*, the contagion would have been *accumulated there*; and must have *infected* the sailors who remained *constantly exposed* to it in a much *greater proportion* than the colonists, they being on shore during the greater part of the day. And, in confirmation of this, it appears from the Memoranda, that when the sailors, in their turns, came to be employed on shore, for several days, (immediately before the Hankey's departure) in cutting wood to serve as ballast, and in filling water for the voyage, these employments (often dangerous between the tropics) were followed by the *deaths* of eight of her crew, and the sickness of a greater number, before she reached Port Praya in the island of St Jago. It must be observed, however, that besides the miasmata to which the Hankey's crew had been this exposed in wooding and in watering, they were soon after subjected to great labour and fatigue, in consequence of her *grounding* on a sand-bank, near the island of Warang,

rang, a circumstance to which Captain Beaver (at p. 192 of his Memoranda) attributes a considerable part of the subsequent sickness.

I have already quoted two passages from Captain Beaver's Memoranda, which prove decisively that he had discarded all belief of the existence of any febrile contagion in the ships or among the colonists at Bulama; and, in addition to these, I will adduce the following extract of his letter to the trustees, written at the time of the Hankey's departure, viz. "The great mortality *must certainly* be attributed to the great labour and fatigue attendant on those who first attempted to settle a colony; and to the necessity we were reduced to of working in the rains, in order to have a fort to defend, and house to cover us. With little strength, we found it necessary to work from morn to night, except when the *rains poured like torrents*; and by these we were often caught when going in the boats, either on board or on shore." Afterwards (at p. 367 of the Memoranda) he says, "I am inclined to think, that, independent of its having really been the *most unhealthy season of the year*, independent of our hard labour, and great exposure during that inclement season, much of our very great mortality may be attributed to the uncommon *depression of spirits* which our situation produced on the minds of the colonists; and I verily believe that I should have died too, if I had ever suffered my mind to be so subdued."

Equally *adverse* to the existence of febrile contagion on board the ships in question, is the testimony of Mr Paiba, who had, in England, been appointed a member of the council at Bulama, and with his wife had lived constantly on board the Hankey, from the time of her leaving England, until her arrival at Grenada. He declares (in the New York Medical Repository, Vol. I. p. 468.) "that the sickness which carried off the colonists, both at Bulama and Sierra Leone, and on the home passage of the Calypso," "was by no means of *one* kind, as the readers of Dr Chisholm would be led to suppose. Few, if any, escaped altogether; some had regular intermittent fever, which is the fever of the coast, of various continuance, from a week to several months; others had a *violent fever*, * which terminated fatally or favourably in one, two, three, four, five, or six days; or which lingered out, after its first violence, as many weeks. Some had diarrhœa and dysentery;

* This violent fever probably occurred in consequence of exposure to heavy rains, drunkenness, fatiguing labour, or exercise in the sun, and other causes, known to produce such fevers between the tropics; either with or without the co-operation of marsh miasms.

dysentery; and others fell martyrs to the indiscreet use of opium and spirits, as preservatives."

Lieutenant-Governor Young, also, in a return which he made of the deaths among the Bulama adventurers, alluding to the fever which had occasioned most of them, observes, that "*the coast fever* is of the *intermittent kind, and not infectious.*" See Wadstrom's Essay, p. 313.

To shorten, however, this discussion, and render all other evidence superfluous, I shall adduce that of Dr Winterbottom, who was, by appointment of the Sierra Leone Company, physician to that settlement, when the Calypso, on her return from Bulama, arrived there with Governor Dalrymple, and the discontented adventurers, and who, besides his personal knowledge of the *African coast fever*, must have had opportunities of making himself well acquainted with that which Dr Chisholm alleges to have been *generated* on board that ship, and thence communicated to the Hankey. And undoubtedly, the *testimony* of a physician so impartial, and so respectable by his character and professional knowledge, as Dr Winterbottom is known to be, must suffice to remove all doubt on this subject. It may be found at p. 16. of the second volume of his history of the Native Africans; where he says, "the fever which carried off so many of the settlers at Bulama, precisely resembled the *endemical remittent fever of Sierra Leone;*"—"but the fever described by Dr Chisholm, *differs so essentially* from that which occurred at Sierra Leone, that it cannot be recognized as the same disease." Afterwards, in the same page, Dr Winterbottom corrects one of Dr Chisholm's erroneous assertions respecting the Hankey, by stating, "that she had no communication whatever with Sierra Leone;" and that "the other vessel, the Calypso, after leaving Bulama, called for refreshments at Sierra Leone, where she remained about six weeks, during which time upwards of forty of the crew and passengers *died* of *remittent fever*, though unattended with any appearance of peculiar malignity." Against such testimony, it might naturally be expected, by persons not aware of the extent of Dr Chisholm's presumptuous *pertinacity*, that he would not venture to oppose any sort of contradiction; *destitute* as he is of all personal knowledge of the coast of Africa, and of the fevers under consideration. But such persons would deceive themselves; for, though Dr Chisholm, even in his last communication, professes to "cherish a very high respect" for the talents, learning, and judgment of Dr Winterbottom, he endeavours, with his usual modesty and candour, to persuade us, that, notwithstanding the long residence of the latter on the African coast, he was not so well able to discriminate and ascertain the true nature of the fever commonly prevailing

prevailing there, and its identity with the fever which occasioned so many deaths on board the Calypso, as himself, (Dr Chisholm) *who never saw one case of fever in Africa, or on board either the Calypso, or the Hankey.* "There is, says Dr Chisholm, (p. 427 of your last volume,) much reason to believe, that Dr Winterbottom *permitted himself* to view the fever, so fatal to the Calypso's people at Sierra Leone, with an *unwary eye*; and that with him, as unhappily with many others, its nature was misapprehended; for, as it is a disease more obvious to the senses than capable of being thoroughly conceived by description, so does it often happen, that we meet with *counterfeits*, and with many respectable physicians, on whom the imposture has been too successfully practised." Now, if there be *much, or any reason* to believe, that Dr Winterbottom had *permitted* himself to view the fever with *an unwary eye*, it was highly incumbent on Dr Chisholm to have stated that reason; but, instead of doing so, he has unguardedly stated *one*, which abundantly authorizes us to reject this most unwarranted assertion; for if, as he says, "it is a disease *more obvious to the senses* than capable of being thoroughly conceived by *description*," Dr Winterbottom is entitled to our utmost belief and confidence, because the disease was, in a multitude of cases, brought under the full cognizance of his *senses*, but never, even in a *single* instance, came within Dr Chisholm's notice; for it must always be remembered, that the Hankey was absolutely without any case of fever when she arrived, and continued so during her stay at Grenada, and that there is not the smallest evidence, or reason for *connecting* the fever which had previously occurred in that ship, with the fever which afterwards became so fatal at Grenada; and considering that the *diagnostics* of remittents and intermittents are so obvious, distinct and certain, that Dr Winterbottom could not have been deceived respecting the fever prevalent on board the Calypso, there is something *worse* than either temerity or folly in the remark which Dr Chisholm has made on this topic.

In the next page (428) Dr Chisholm makes another attempt to invalidate Dr Winterbottom's testimony. "In a mortality (says he) of about one in four, of the whole of the people on board the Calypso, occasioned by this fever, the absence of *any appearance of peculiar malignity* is astonishing indeed; and is, I apprehend, totally irreconcileable with the local circumstances of Sierra Leone, as described by Dr Winterbottom himself, and with those of Bulama, as we see by the descriptions already referred to; or it must be considered as furnishing a decided proof of the deceitful nature of the disease itself." He then refers to Dr Winterbottom's description of the soil in the neighbourhood of

of Freetown, which is represented as being, "not incommoded with swamps in its vicinity;" and he seems to imagine, that he is warranted, by this circumstance, to infer, that Dr Winterbottom was completely mistaken, and that, notwithstanding the notoriously frequent occurrence of intermittents and remittents at Sierra Leone, long before the arrival of the Calypso, no such diseases, nor any miasmata to produce them, could ever have existed there. He has, in other places, attempted to apply this reasoning to Bulama, where no *distinct formal swamps* or marshes have been noticed. But if he really does not know that these are far from being necessary for the production of miasmata sufficient to produce in *hot climates* the very worst of intermittent and remittent fevers, his ignorance is most disgraceful; and, to avoid a farther waste of time, I must advise him to extend his reading, and, among other books, to peruse with attention the third and fourth parts of my Essay on the Yellow Fever, with the note subjoined to page 694 of that volume, which contains facts more than sufficient to remove all doubt on this subject.

When the Hankey had been a week at Port Praya, as lately mentioned, the Charon ship of war, bearing the flag of Commodore Dodd, arrived there from England, as did the Scorpion some days later. From the testimony of Mr Paiba (in the 1st Vol. of the New York Medical Repository), it appears, that, while the Hankey was at this island, there was no sickness on board, excepting "debility, and slight intermittents;" that her Captain, (Cox), as well as "her crew, and passengers, mixed without suspicion, and with perfect freedom, with the inhabitants of Port Praya, and received them on board, where they had a number of entertainments, of which the governor of the island, and several of the principal people partook," without its having ever been afterwards suspected that any sickness had been "excited" by this intercourse. "Indeed, (says Mr Paiba,) no sickness prevailed at St Jago during the Hankey's stay, excepting the common ague and fever of the place." It is added, that, during the ten days which the Charon remained at Port Praya, it was the commodore's custom to send his barge every morning to the Hankey for Mr and Mrs Paiba, who usually spent the day with him, and returned to the Hankey in the evening; that Captain Cox was several times on board the Charon; that Mr Smither's her surgeon, and his mate, visited the Hankey, and two of the Charon's seamen were employed upon the Hankey's rigging; and finally, that Commodore Dodd had so little apprehension of danger from infection in the Hankey, that he gave Mr and Mrs Paiba a letter of recommendation to a gentleman in Grenada, by whom, upon the Hankey's arrival, they were invited to reside,

side, and did reside at his plantation, during their continuance on the island. It appears also, that, as the Hankey, in consequence of her previous loss, was but weakly manned, Captain Cox obtained two seamen from the Charon, and the like number from the Scorpion; and, with this addition to her crew, the Hankey left Port Praya. It appears, however, by her *log-book*, that one, and only one, of the seamen sent from the ships of war, viz. Samuel Hodge, died on the 4th of February. But Mr Paiba declares, that this man "was unwell when he came on board," though "able at that time to do duty;"—that he grew more and more unwell as they proceeded. He adds, "that Captain Cox, who had been unwell when the Hankey left St Jago," recovered his health before they reached the West Indies; though he afterwards had a return of his disorder, (an intermittent), and all the others were *perfectly well*, (notwithstanding the hard duty they had to perform), and *continued* so. After a passage of nineteen days, the Hankey reached Barbadoes; and there Mr Paiba asserts, that, during a great part of two days, her Captain, passengers, and crew communicated freely with the inhabitants, without any suspicion of having caused any disease among them. This they also did at St Vincent, and arrived at Grenada on the 18th of February.

From this statement of facts, which accords with the Hankey's log-book, and is, besides, well *ascertained*, it must be concluded, that there was no room to suspect the existence of any febrile contagion on board the Hankey, when she reached Grenada, though Dr Chisholm appears, soon after, to have employed unusual industry to propagate the most alarming reports concerning this ship. Of these reports, or rather *positive*, though groundless, *assertions*, I shall notice only those stated at p. 87, *et. seq.* of his first publication, respecting "the malignant pestilential fever, (as he called it) from *Boullam*." "On the *third* day after leaving St Jago, says Dr Chisholm, the men procured from the ships of war were seized with the fever which had carried off three-fourths of those on board the Hankey at *Boullam*; and, having no assistance, two of the four died; the remaining two were put on shore *here* in the most wretched state possible." He adds, "that Captain Dodd of the Charon, coming to Grenada on the 27th of May, and hearing of the mischief which the Hankey had been the cause of, mentioned, that some of the Charon's and Scorpion's people were sent on board the Hankey at St Jago to repair her rigging; and that, from this circumstance, and the communication which his barge's crew had with that ship, the *pestilence* was brought on board both *ships*; and that of the Charon's crew *thirty* died, and of the Scorpion's, *fifteen*." This most

mischievous

mischievous falsehood was excellently adapted to support a belief of the alleged introduction, by the Hankey, of a most direful pestilence at Grenada; and, for some time, it seems to have been implicitly believed. Fortunately, however, for the cause of truth, and the interests of medical science, its turpitude was, after some time, detected and exposed by Dr Trotter, surgeon to the Vengeance, of 74 guns, one of Admiral Gardner's squadron, then convoying the West India summer fleet to England. He states, (at p. 227 of the 1st Vol. of his Medicina Nautica,) that, on the 22d of August, a ship, one of the fleet, "lost her foremast in a squall of wind, and received other damage;" upon which "the Admiral made signal for the Vengeance to take her in tow. The ship proved to be the Hankey from Grenada and Bulama. Captain Thomson sent carpenters on board, with the necessary stores, to assist in repairing her losses; they remained for *three or four days, but no sickness followed.*" Dr Trotter afterwards mentions, how, by this and other circumstances, he was induced to *inquire* concerning the truth of Dr Chisholm's account of the mischief said to have resulted from the Hankey's intercourse with the Charon and Scorpion; and particularly of Captain Dodd, who "had *his broad pendant*" in the former ship, and of Mr Smithers, her surgeon; and that, from these gentlemen, he had obtained a narrative of their transactions with the Hankey, of which he gives a copy. By this narrative, it appears, that, when the Hankey was at Port Praya, the fever had been "overcome,"—that "Mr Smithers saw two men who had lately recovered,"—that he prescribed for "a venereal complaint" in the master, and left him some mercurials and bark: That "the Hankey at this port was cleaned, washed with vinegar, and fumigated:" And that "no fever appeared in either of the men of war in consequence of this communication; they arrived at Grenada in perfect health," &c. Dr Trotter adds, "It is probable, from these facts, that the Hankey did not import the infection that produced the Grenada fever." He moreover observes, that the Hankey's bedding had been thrown away, and what clothing remained had been aired, and probably had scarcely been in contact with the body after being sick:" That Mr Smithers was examined before the (Lieutenant) Governor of Grenada on the subject, "and *gave his opinion decidedly, that the Hankey did not communicate this fever to the colonists.*"

It has moreover been ascertained, from the log-books of the Charon and Scorpion, that the latter ship did not lose a single man during her whole voyage, and that the Charon only lost *four*, from causes described by Dr Trotter, *and wholly foreign to the Hankey*; even of these, two did not belong to the Charon, and

and one was a black prisoner sent to England to be tried for murder.

Should it here be asked what excuse Dr Chisholm has made for statements so groundless and mischievous, the only answer to be given must be, that, in his preface to the second edition of his work (in 2 Vols.) after noticing Dr Trotter's publication, he has said, "on further inquiry, I find I have been *incorrect* in my *statement*," merely "*incorrect !!*" Would Dr Chisholm have applied this gentle epithet to me, if I had made a positive statement of *so much importance*, without the smallest foundation in, or particle of, truth? He has also said, that having been "*politely* favoured" with "the perusal of a log-book" kept by a "lieutenant of the Charon, during the voyage in question," he had "found that no sickness took place in consequence of the *interview* between that ship and the Hankey; that "a *log-book* is unquestionable evidence, and therefore" (he adds) "I have *suppressed* what I have advanced on this affair." Such is the *atonement* which Dr Chisholm has thought *sufficient* for his trangression. After an interval of some years, he *omits* to *republish* his untruth respecting the alleged communication of a pestilential fever from the Hankey to the Charon and Scorpion, and the pretended deaths of no less than forty-five persons in consequence thereof. But he does not resolve upon this *omission*, until, in addition to the evidence adduced by Dr Trotter, he found himself convicted by that of a log-book. And, even then, his *omission* extended merely to the pretended sickness and deaths on board the Charon and Scorpion; for he has most unwarrantably reasserted, and republished every other part of his groundless statement, including that of the alleged communication of pestilential fever to the *four* seamen supplied to the Hankey from the ships of war, and its having caused the deaths of two of them. Probably he felt the necessity of alleging something like evidence, of the existence of this direful contagion on board the Hankey, to justify his accusation of that ship; and though the accusation would have been supported more triumphantly, by the *forty-five pretended deaths* on board the ships of war; yet when the "unquestionable evidence" of a log-book made it absolutely necessary to relinquish these, it might be hoped that the others would suffice.

Dr Chisholm finally intimates, that the untruths which he had thus omitted to republish, were *advanced* by him on the authority of Mr Home, then Lieutenant-Governor of Grenada; adding, "but why Mr Home should mention this as a fact, communicated to him by Commodore Dodd, I can assign no reason." This I believe! but instead of an inquiry about the "reasons" which

which induced Mr Home to advance these untruths, it would have been much better if Dr Chisholm had endeavoured to make it probable, at least, that Mr Home did *really* advance them. This was not even pretended by Dr Chisholm, until after Mr Home had lost his life by the hands of assassins, and with it the possibility of defending his reputation. On the contrary, Dr Chisholm's positive statement was made as from his *own knowledge*; he alone stood forth as *avouching* its truth, without the slightest allusion to any other authority. In regard to Commodore Dodd, it must be *absolutely incredible* that he could have invented falsehoods, which he manifested so much readiness to *contradict*, or that he would have blasted his own character, by assertions of too much importance to pass without investigation, and which the log-books, both of the Scorpion and Charon, as well as the testimony of the officers and crews of these ships, would prove to have been groundless; and, especially when his own surgeon, who was examined *at that very time by Mr Home*, from the decided opinion which he is stated to have delivered on this subject, *must* have declared, that no sickness had been produced in the ships of war by their communication with the Hankey; and certainly I cannot believe that Mr Home, after receiving such a declaration, would have falsified the truth, by making the statement which Dr Chisholm *now* imputes to him. Had he done so, it is highly improbable that Dr Chisholm would have been the *only person to whom it was made.* The latter must obviously have had an urgent motive to exculpate himself, by this imputation; but none is discoverable which could have induced Mr Home to give occasion for it. When, therefore, I am reduced to the necessity of considering either Mr Home or Dr Chisholm as having *invented* this falsehood, I feel no hesitation in acquitting the former, especially as his veracity does not appear, in any other instance, to have ever been impeached, *and as Dr Chisholm cannot pretend to this advantage.* See in regard to the *latter position*, a letter from Mr T. F. Eckard, Danish Vice-Consul at Philadelphia, in the 1st volume of the second hexade of the New York Medical Repository, (p. 337); and in the seventh Appendix to my Essay on the Yellow Fever, (p. 719).

To conclude this subject, I will now proceed to the alleged diffusion at Grenada of a most *direful* contagion by the Hankey. In order that such diffusion should have been possible, this contagion must have previously *existed* on board the Hankey. But of such existence there is not the slightest evidence or probability; but, on the contrary, a multitude of facts and reasons conspire to manifest the absurdity and fallacy of this allegation. The coast of Africa whence the Hankey had sailed, and where it is pretended

pretended that this contagion was generated, is, of all parts of the globe, the least likely to have occasioned or favoured a production so monstrous and incredible; there being no ground for believing that a *contagious fever ever occurred there*; the fevers which had occasioned so much mortality on board that ship, being, as has been proved, of a very different origin, and destitute of contagion. But, had the case been otherwise, the *superfluous* precautions taken by Captain Cox, must have rendered his ship harmless, for "before the Hankey put to sea (from Bulama,) all the bedding of the sick was thrown overboard or destroyed, and the ship was washed from stem to stern, both above and below, with salt water, and then with vinegar and water; and the purification was completed by thoroughly fumigating her with tar, pitch, and gunpowder," and this purification was *repeated* at Bissao, (see New York Medical Repository, Vol. I. p. 466.) Moreover, it appears, as has been already stated, on the authority of Mr Smithers, that the Hankey was a *third* time "cleansed, washed with vinegar, and fumigated," after her arrival at Port Praya; and that no sickness resulted to any of the great numbers of inhabitants who there visited and were entertained on board her, nor to the ships of war, nor the seamen obtained from those ships; nor to the inhabitants of Barbadoes and St Vincents, who communicated with the Hankey, immediately before her arrival at Grenada; and that all her crew and passengers were in health when she arrived. That no sickness occurred in that ship during her stay at Grenada, or in her subsequent passage to Europe, either among her original crew, or the *six* additional seamen obtained there for the voyage; or to the carpenters from the Vengeance, who remained on board several days, as mentioned by Dr Trotter. In truth the Hankey's condition, and the *facts demonstrating that condition*, were such, that, even if there had been room to suspect an importation of febrile contagion at Grenada, this ship, of all others, ought to have been deemed the least liable to suspicion. But though, from the condition of the Hankey, her passengers and crew, it must have been difficult to fix upon any thing belonging to her as capable of retaining and diffusing the supposed contagion, some persons discovered that the more valuable part of the clothing and effects of the persons who died at Bulama, had, after being aired, been replaced in their respective trunks, or chests, and retained (I believe) in the Hankey's *hold*. *Thus circumstanced, and innocuous* as they were found to be at all places, and to all persons before the Hankey reached Grenada, these effects ought not to have been suspected, even if it had been ascertained that the prevalent fever at Bulama was possessed of a contagious

quality;

quality; they being so *confined and shut up* as to render any *diffusion* of contagion from them utterly incredible. But neither this consideration, nor that of the total want of any fact to prove that contagion had ever existed on board the Hankey, were sufficient to restrain Dr Chisholm from representing these effects as *pestiferous*. A violent yellow-fever occurred at Grenada, and in other parts of the West Indies, as had repeatedly happened before, *from local causes*, and it suited the purpose of Dr Chisholm, to consider this as an *imported* disease, a *nova pestis*: and to charge its importation upon the Hankey, and more *immediately* upon the effects lately mentioned; though the clothing and effects of Mr and Mrs Paiba, including a large quantity of merchandise, were actually *landed* at Grenada, and deposited at the house of Mr Napier, in the town of St George, without even a suspicion of their having ever produced any disease. Dr Chisholm, and his few adherents at Philadelphia and New York, contend, *obstinately*, that the violent yellow-fever which prevailed at the former of these cities in 1793, was *identically* the same as that which they allege to have been derived from the Hankey, and that it was produced by the same specific contagion. Now it is notorious, that, *for this fever exclusively*, an hospital was in that year opened at Bush Hill, near Philadelphia; and that after the fever had ceased, at the approach of winter, the bedding, blankets, and clothing, which had been used by great numbers of patients in *succession*, were all sold to the agents of the French government, and employed for the sick on board their ships of war, then in America, without having been *washed, fumigated, or even aired; and that no disease was thereby produced.* Respecting this fact, see Dr Valentine's "Traité de la Fievre Jaune," p. 92, 93. Indeed, a multitude of proofs might be adduced of the perfect innocency of bedding and clothing, after having been used by persons labouring under the fever, which, as Dr Chisholm pretends, was derived from the Hankey, and afterwards conveyed to different parts of America, Spain, &c. Nor can it be made in any degree probable, that this fever ever was communicated by bedding, clothing, &c. in a single instance. Even the Calypso, in which, as Dr Chisholm asserts, this direful contagion was originally *generated*, did not infect any of the inhabitants at Sierra Leone, during the six weeks which she remained there, even with many sick on board; nor was any fever produced after her return to England, by her surviving passengers, with their clothing, or by the clothing of nearly 50 persons belonging to her, who had died; though this was all landed and distributed to those who were entitled to it. Nor does it in any way appear, that the clothing and bedding of those

those who died of the fever under consideration *in Grenada*, or *even the fever itself*, ever did reproduce the disease, in a single instance. They were indisputably incapable of doing it, in any of the cases of persons who, after being exposed to the local causes of the disease, in the town of St George, sickened in places beyond the reach of those causes, *i. e.* in the country. From such persons or their bedding, &c. *no communication of disease ever took place**; and, in every case where persons were supposed to have derived the fever from others labouring under it, they had previously been exposed to the same *local* causes which first occasioned it in others. This was also the fact, not only in Grenada, but in every other part of the West Indies, North America, Spain, &c. as I have abundantly proved in my Essay on the yellow-fever. To imagine, therefore, that the clothing of persons who died of fevers *void of contagion*, (intermittents and remittents,) even when *shut up* in trunks, &c. and deposited in the hold of a ship, could, and did produce effects, which other clothing, a thousand times more exposed to morbid contamination, *is absolutely incapable of producing*, must be highly absurd. Dr Chisholm has, however, at different times, confidently described the persons, and the channels by which the supposed contagion of the Bulama baggage was communicated and diffused at Grenada. According to his original statements, "a Captain Remmington, an acquaintance of Captain Cox, was the first person who visited the Hankey after her arrival in St George's Bay;" and, in consequence of that visit, "was seized with the malignant pestilential fever." And "the crew of the Defiance of Blythe Port, were the *next* who suffered by visiting the Hankey," which they are said to have done on "*the day after her arrival.*" To give some plausibility to these statements, Dr Chisholm connected them with various pretended facts and circumstances, the fallacy or non-existence of which were so plainly demonstrated in my 7th Appendix, (p. 725, *et. seq.*) that he has thought proper *wholly to omit them* in his last communication, and to substitute, (at p. 419,) as the persons *first infected*, the "sailors" belonging to certain *nameless* ships, "who assisted in working the Hankey into the mouth of the carenage;" they "sickened, and communicated the disease to their respective ships." By this substitution, Dr Chisholm has not only omitted, but *contradicted* his former statements on this subject; for the visits, by which Captain Remmington and the crew of the Defiance were said to have imbibed contagion, were made, according to his account,

* For proof of this, see the 7th Appendix to my Essay, p. 759, 760, et seq.

within 24 *hours after the Hankey's arrival, and while she lay in the Bay;* and, in the same account, it is asserted, that she "*for a few days lay in the bay,* but was *afterwards* brought into the carenage." Consequently, if the sailors who assisted in bringing the ship into the carenage, *were the first* by whom the contagion was imbibed, Captain Remmington and the crew of the Defiance could not have imbibed it *some days before.* Should it be asked how Dr Chisholm acquired the knowledge of facts so incompatible with his former repeated statements; his answer is, they "were stated to *me* by the (nameless) captains of the (nameless) ships in which this dreadful calamity took place." But, if so, why were they concealed during 20 years, and *different accounts published?*

In the same page, Dr Chisholm asserts, that, "a Bermuda mercantile house, (Messrs Wood and Company,) first received the infection on *shore.*" "I attended the family, (says he) and *could not be deceived, had there been a design to deceive.*" *What presumptuous, unwarranted assertion!* He adds, "A negro woman belonging to the house took in washing from the shipping, and, from *the foul infected linen* thus brought into the family, the disease was communicated to the members of it." From such confident positive language, and from his reference at p. 421 to the *evidence of our senses,* as proving that the Grenada fever was "a fever of infection derived from the ship Hankey," it might be reasonably supposed, that Dr Chisholm had ascertained, that this foul, and, as he calls it, *infected* linen, was actually sent from the Hankey, or from one or more of the ships supposed to have received contagion from her; and that he was at that *time* possessed of more than *microscopic eyes,* or some *supernatural* acuteness of *smelling,* &c. which enabled him *distinctly* to perceive and ascertain the *presence* of contagion therein; and that he had actually subjected the linen to the most energetic operations of these *supernatural senses,* and thereby had made it *impossible* that *he* should "*be deceived,* had there *been a design to deceive.*" I find, however, from his *original* account, that he never saw the linen in question, or had any knowledge of the ships whence it was sent; and that they might have been, and probably were, free from sickness; in short, that he had no sort of foundation for believing that this linen was infected, excepting the subsequent occurrence of fever in the family where it was washed; although, from the places and manner in which washing is performed at Grenada, it is highly probable that the *white* part of the family, who alone had the fever, never saw or approached the linen in question. It will not, I presume, be expected or desired, that I should pro-

long

long this discussion, by *distinctly* refuting Dr Chisholm's *less* important, but equally fallacious statements on this subject; they having been abundantly refuted in my 7th Appendix, to which I must refer those who may be disposed to investigate the subject more minutely. Nor will it, I hope, be expected, that I should condescend to bestow much notice on those of his observations which are directed against me personally, such as that at p. 415, *et. seq.* of my having "bestowed immense labour, and displayed a blaze of talent and erudition, to prove what has *never* been denied by intelligent and well-informed medical men;" *i. e.* that "the yellow remittent fever, in all its grades, and in all countries where it appears, is the product of *local causes*, and perhaps universally of marsh miasmata."

If, as I conclude, Dr Chisholm means to say, that until the appearance of his supposed "*nova pestis*," or malignant pestilential fever, in 1793, the ordinary yellow-fever of America had never been thought to proceed from contagion, by "intelligent well-informed medical men," he must exclude from that number many of the most eminent physicians, particularly Dr Henry Warren of Barbadoes, Dr Lining of South Carolina, Sir Gilbert Blane, &c.; and he must also have forgotten that it had been strongly contended by the late Dr George Fordyce, and other physicians of high reputation, (mentioned at p. 88. of my Essay), that "*all fevers whatever*," even the intermittent, are contagious; and the arguments, and supposed facts which had been then recently adduced by Dr Fordyce, to support this doctrine, remained unanswered, until the publication of my Essay.

In the same page (415) Dr Chisholm observes, that, "whereas my predecessors urge their attacks in a neat comprehensive form," I have "diffused myself over 811 pages of paper;" as if my volume had *all* been employed upon him and his writings; when, in fact, excepting my 7th Appendix to that volume, but very inconsiderable portions of it have any relation to Dr Chisholm; and those parts which do relate to him, are very far from being *diffused*, as *he must have felt*, under that "*unconquerable indignation*" which he declares them to have excited.

Dr Chisholm has, in *one* place, blamed me for having "adduced *no* authorities but those which favour my own opinions;" and in another, for having, when I quoted one of Captain Beaver's letters, omitted a part, which was wholly unimportant. But if I had done otherwise, and extended my work by useless or inapplicable quotations, the number of pages, of which he complains, and which I laboured earnestly to diminish, must have been much greater.

At p. 416. Dr Chisholm endeavours to assimilate himself to

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" the *humble* but *undaunted* Mordecai ;" and myself to the " *wicked* Haman ;" though I believe that *wickedness* is as far removed from my character, as *humility* is from that of Dr Chisholm ; and farther I need not desire it to be. Indeed I had never before supposed the epithet of *humble* to be applicable to this same Mordecai, though, like Dr Chisholm, he appears to have been *sufficiently undaunted.* I will not, however, waste my time, by noticing any more of Dr Chisholm's frivolous observations ; but I cannot entirely pass over *three* letters which he seems to have taken some pains to procure, and subjoin to his last communication ; though, in regard to the points in dispute, they only confirm what I had previously stated in my 7th Appendix ; viz. that most exaggerated and fallacious reports had been industriously propagated at Grenada respecting the Hankey, and that, in consequence thereof, it had been there generally believed, that the fever of 1793 was produced by the contagion, supposed to have been imported by that ship.

In the first of these letters, written by K. F. Mackenzie, Esq. (who, in 1793, was attorney-general of Grenada,) the writer says, " of the infectious and distressed state of the Hankey, at the time of her arrival at Grenada, in February 1793, I can speak only from report." Of this report he gives account, repeating therein the fiction respecting the seamen put on board the Hankey by Commodore Dodd ; but with this variation that they were *three in number*, and that they *all died.* He adds, that " the disease first appeared in a vessel (the Defiance) with which the *people* of the Hankey had had communication, at the innermost and windward end of the shipping, in the carenage ;" *i. e. as far as possible from the Hankey* ; and here it is stated, that the *Hankey's people* communicated with the Defiance, and not those of the latter with the former ; consequently the Bulama baggage, to which Dr Chisholm has ascribed all the mischief, must have been at a great distance, and *quite innocent.*

Mr Mackenzie proceeds to mention his having (with Mr Beete) visited Mr Sharp, when the latter thought himself dying ; and adds, " the *next* day, we felt symptoms of the disorder :" and the day following, " were confined to bed, and reduced to a state of infantine weakness ;" and he declares that he has no doubt of his and Mr Beete's having " both received the infection from Mr Sharpe." By thus assigning his reason for believing that he and his friend received the fever by contagion, Mr Mackenzie enables me to ascertain the *fallacy* of his belief ; there being no contagion as yet known, which occasions such speedy reaction, as to produce symptoms of disease within 24 hours. Mr Mackenzie and Mr Beete lived under *one* roof, and having been previously

previously exposed to the same local causes of the fever, they sickened about the same time, as might be expected, without any infection from Mr Sharpe.

Mr Mackenzie further says, "I firmly believe, though my opinion, *not being a medical man, can be of no importance*, that the great mortality was the result of *contagion*, and not of *marsh* miasmata. To the *first*, there was *much exposure*; to the last *none*." That Mr Mackenzie believed these latter assertions, I have no doubt; though, as a lawyer, acquainted with the *nature* of evidence, it might have been expected that he would have discovered that they were supported by *none*. Contagion and marsh miasmata being alike *imperceptible by the senses*, it must have been impossible for him to know, that a single particle of febrile contagion then existed at Grenada, or that an abundance of what are called marsh miasmata, did not exist there; and in fact we have the best reason, short of positive proof, for believing the facts to have been *in direct opposition* to Mr Mackenzie's belief.

The writer proceeds to state, that "mercury was used, and in *many* instances, as I have understood, effected a cure; particularly when it affected the mouth." "I know, (says he) that you thought favourably of mercury, as a remedy, especially when *salivation*, or an approach to it, was produced." He then mentions four cases which occurred at his estate in Demerary, as being "*corroborants* of the correctness of Dr Chisholm's opinion;" for *all the four patients took large quantities of mercury*, by the mouth and by friction; but *three*, who had not their mouth affected, *died*, while the fourth, in whom ptyalism was produced, recovered." This is certainly a strange proof of the beneficial effect of mercury, in the fever under consideration; and agrees very nearly with Dr Chisholm's account of his success at Grenada, where, out of 26 patients (belonging to the Royal Artillery) 21 *died*; but with this difference, that, of the *five* who survived, two appear to have taken no mercury, and two of the remaining three to have taken it very sparingly. Mr Mackenzie, however, seems to suppose that *all* the cases on his estate would have proved fatal, if mercury had not been given; and to have forgotten the great numbers who recovered under the hands of the other practitioners in Grenada, and elsewhere, who refused to employ that dangerous remedy. Had Mr Mackenzie looked into my Essay, he might have found, between pages 76 and 86, an adequate cause for the more frequent recovery of those in whom mercury produces salivation, in the fact stated by Dr Rush, and well known to others, viz. that mercury, even when most freely given, does not *salivate* until the fever *declines* or *intermits*. The occurrence, therefore, of this effect, indicates not, (as has

been

been pretended) that mercury operated favourably upon the disease, but that the fever having, from *other causes, declined*, or *intermitted*, had allowed the supposed remedy to act as usual, as it commonly does, upon the salivary glands, in doing which, it certainly produces effects always troublesome, and often highly detrimental to the patient, *without any certainty* or probability of its having ever done the least good, in the fever under consideration. Mr Mackenzie adds, "In your own case, I am perfectly satisfied from the recollection of circumstances, and from conversations by Dr Monro, and Mr Campbell, that you took mercury largely; but that matter may be reduced to certainty, by applying to Dr Monro." Here Dr Chisholm interposes the following passage, viz. "I request the reader's attention to a note of Dr Bancroft, p. 83. of his Essay, where he accuses me of falsely stating the result of my own case. Mr Mackenzie's obliging statement must be considered as an ample refutation of this absurd calumny. I expect, however, a letter from Dr Monro, which I trust will exhibit the malevolence of this writer in its true light." Now, it so happens, that, in the note to which Dr Chisholm here refers, I stated, not that Dr Chisholm did not take mercury, but that, after having "had recourse at first to mercury, he became convinced of its inefficacy, in his own case, early enough to call for the advice of Dr William Monro, then of Grenada, and now of Demerary, under whose care he recovered, but, as I have been well *informed*, by a very different mode of treatment." This information I obtained from my father, to whom it was communicated in the year 1805, by *Dr Monro himself*; and I have no apprehension of its being contradicted by the latter, whatever pains Dr Chisholm may take to procure such contradiction: nor has it been, in the slightest degree, contradicted by Mr Mackenzie's "obliging statement," as Dr Chisholm would make his readers believe: on the contrary, that statement affords good ground to infer, that Dr Monro was called in, as I had mentioned; but it affords none for supposing that he afterwards prescribed mercury for Dr Chisholm; and Dr Monro positively assured my father, that he had carefully abstained from any such prescription, because he did not believe it to be a proper or safe remedy in that fever; and, therefore, when Dr Chisholm ventured to represent Mr Mackenzie's statement, as being "an ample refutation," of what he calls my "absurd calumny," he was by *much too* "*undaunted.*"

The second letter, written by James Baillie Esq., then of the house of Baillie, Thornton, and Campbell, at Grenada, contains a repetition of the reports which I formerly stated to have been propagated respecting the Hankey; but without any thing which could

could be considered as affording the slightest *evidence* upon the points in dispute.

The third and last of these letters is from Lieutenant-General Oliver Nicolls, formerly Lieutenant-Colonel of the 45th regiment, and it recites *five* questions put to him, in a previous letter from Dr Chisholm. The first of these relates to the circumstances in which the Hankey arrived at Grenada in February 1793; to which the Lieutenant-General answers, "*I was not in the West Indies in* 1793, but have heard from a gentleman of *veracity* and *consequence*, that the Hankey arrived in a *dreadful sickly state*,"—an answer which shews how grossly these gentlemen of *veracity* had allowed themselves to be deceived, and under what fallacious impressions the Lieutenant-General himself wrote his letter; for he appears, whilst writing it, to have supposed that this was a true account of the Hankey's condition.

In the second question he asks, "what was the progress of the fever from that ship (the Hankey) to the other shipping in the carenage, and to the inhabitants of the town and garrison?" Answer, "*I was not there*, as I have stated above; but I understand, from the time the fever began among the shipping, its progress was alarmingly rapid, and destructive." But how does this connect the fever with the Hankey? Probably after such answers, to the two first and most important questions, I shall not be expected to notice the others; especially as the Lieutenant-General, by not having been in the West Indies, had no personal knowledge of any of the matters in dispute, and could only answer upon the ground of most fallacious reports, to which, like many others, he gave unmerited credit. It may indeed be observed, that he expresses, in his fourth answer, a belief of his having, two years afterwards, taken the fever, by "having in *one* day gone to visit *two* officers ill of it;" because, on "the *same* night, he was himself attacked." Here, as in Mr Mackenzie's case, the mentioning of his supposed ground for believing the fever to have been contagious, proves the want of solidity in that ground,—no contagion having ever as yet been known to produce *manifest disease* in so short a space.

The reader need not, I think, be told, that Dr Chisholm must have thought his case desperate, when he made this abortive effort to bolster up his reputation; and, abusing the complaisance of his friends, induced them to *appear* so disadvantageously before the public, for the purpose of repeating such groundless, and now exploded reports *upon mere hearsay*, and of delivering opinions, which, from men destitute of *medical* science, could be of no value, upon one of the most intricate and fallacious of *medical* subjects—a subject upon which many eminent physicians have been

been deceived. Witness the late Dr Rush, who for several years believed, and endeavoured to make others believe, that the yellow-fever at Philadelphia (derived, as Dr Chisholm pretends, from the Hankey) was contagious, though he afterwards discovered his error; and, having done so, he with the most laudable candour, and regard for truth, took the earliest opportunity of retracting, and counteracting the effects of his former opinions: adding, "I am aware of the influence *which such changes in medical opinions, as I have acknowledged*, have upon a physician's reputation; but *small* indeed should I consider the *total sacrifice of mine*, could it avert the evils which are connected with a belief in the importation of pestilential diseases." And, in another publication, after referring to this retraction of his opinion of the supposed contagious quality of yellow-fever, Dr Rush, "*begs forgiveness of the friends of science and humanity, if the publication of that opinion has had any influence in increasing the misery and mortality of that disease.*" Indeed, such is the *pain he feels* in recollecting that he ever entertained or propagated it, that it will "long, and perhaps always, deprive him of the pleasure he might otherwise have derived from a review of his attempts to fulfil the public duties of his situation." How magnanimous! how admirable is this conduct in Dr Rush? and how worthy of imitation by Drs Chisholm, Haygarth, and Hossack? I fear, however, that some of these gentlemen, rather than make a similar *avowal*, and *atonement*, would gladly, if possible, sustain their misrepresentations, though at the risk of perplexing and confounding the most important medical truths, and of misleading physicians in their reasonings, and methods of treating the disease under consideration, as well as governments, in regard to the measures most suitable to obviate the calamities which it occasions; and this apprehension could alone have induced me to undertake the painful task, of enabling the public to *estimate justly* the value of Dr Chisholm's assertions, employed, as they have been, most unrestrainedly and unwarrantably.

VIII.

VIII.

Case of Phymosis in a Boy, brought on by tying a Thread around the neck of the Glans Penis. By H. Thomson, Esq. Surgeon, Paisley.

I Send you a short account of the following case, partly as a matter of curiosity, trusting, however, that it may lead to some practical usefulness, as it will once more remind the profession of the absolute necessity of closely investigating every circumstance before they come to a final conclusion.

B. aged —, the son of a gentleman in this place, was reported to me by his father to have an immense swelling, with most acute pain, of the glans penis, which prevented him from discharging his urine with freedom; that, during the night, he had frequent erections, which made the boy scream out with agony; and these disagreeable symptoms had been of some weeks continuance, and seemed still increasing. Supposing the disease to arise from an inflamed state of the glans, as the prepuce could not be retracted, I ordered a solution of sugar of lead to be applied constantly on a piece of linen during the day, and the same to be continued by way of poultice through the night. This practice was persevered in for some time, without any apparent amendment of symptoms, while tepid water was injected between the glands and prepuce. To allay irritation, and prevent the disagreeable effects arising from the erections, which, from the description, had all the appearance of chordee, I gave a few drops of tinct. opii at night, which produced the desired effect; but the swelling of the glands and tightness of the preputium continued unabated. I now saw him for the first time, and found the parts in the exact situation as they had been described. The prepuce was so firmly pressed around the glands, that it was found quite immoveable, and, indeed, seemed ready almost to burst, so completely was it filled. On even attempting to move it backwards but slightly, the boy screamed out, and I was obliged immediately to desist. At that instant, I would have had recourse to circumcision, or slitting up the prepuce, to get at the root of the matter; but having oftener than once had occasion to witness the very bad effects arising from both these operations, in venereal cases, I determined to wait a little longer, and continue my former plan of treatment. I gave slight doses of calomel; repeated them every day, and remained in expectation of some favourable

favourable result. About a week had elapsed, when one day, in the presence of the boy's father, I attempted to pull back the prepuce; it resisted, and he complained of severe pain. I thought, however, it felt softer, and that it seemed to move with more freedom than before. Still there was a danger, should I succeed, of producing paraphymosis, a situation of parts, perhaps more disagreeable than that in which they now stood. Anxious to see what was beneath, I made an effort, and perceived a small white speck, in appearance similar to matter, and lifting it with the points of a pair of blunt forceps, I found it to be a piece of strong thread, which resisted every effort to bring it away. I then pushed back the prepuce at all hazards, and saw, to my astonishment, a double linen thread tied firmly around the neck of the penis, immediately above the corona glandis, and which had cut nearly through the whole body of the yard. Indeed, but for the resistance made by the urethra, and the slackening of the thread, as it continued its way inwards, the whole glans must have by this time come away. As it was, the frœnum was completely divided; the glands suspended to the body of the penis by little more than the thickness of the urethra. The constant irritation kept up by the thread, even when it came to slacken, prevented the swelling of the glands from diminishing, and the consequent retraction of the prepuce. After dividing the thread, the parts healed up of themselves, in the course of a few days, and with a very little attention, by frequently slipping the prepuce over the glans, stretching it forwards, and retaining it in that situation with the fingers. Every thing soon resumed its proper and natural situation, with the exception of the frœnum, which now remains completely divided.

On inquiry, I found that one evening, when the boys were undressing themselves, the maid was so employed as required her to cut away the ends of a number of threads, which she threw down on the floor around her. The boy had amused himself with rolling these around the point of the penis; one of them had unfortunately got into the situation which I have described, and he recollected nothing of the circumstance till the discovery was made.

IX.

IX.

Case of Rheumatism of the Heart. By J. Penkivil, Esq. Surgeon, Plymouth Dock.

The susceptibility of the heart to take on inflammation, in patients afflicted with acute rheumatism, though no doubt observed by authors of more ancient date, yet seems more particularly to have attracted the attention of the practitioners of the present day. We have several cases mentioned in the volumes of the Medico-Chirurgical Transactions; and an interesting case is communicated by Mr Russell, in the last number of the Edinburgh Medical Journal. Those who are in the habit of frequent dissection, often observe also, that this viscus appears to have been inflamed, where it was not particularly indicated by any previous symptoms. It is indeed wonderful, that an organ so irritable, and so essential to life, in which may be almost said to originate the principle of vitality, should ever be affected with active inflammation, without a complete, and even speedy extinction of life. This would be the reasoning *a priori*. But when the heart has been wounded by a sword; when a bullet has been lodged in one of its ventricles, and yet its functions preserved;* when the sensorium itself has been wounded, and even a considerable portion of its substance removed, and yet the patient survive, our astonishment lessens at what may appear less injuries; whilst it is again excited by the consideration, that a slight puncture in the extremity of a nerve will produce inflammation and mortification,—and a mere scratch induce convulsions, tetanus, and death!

I have been led to these remarks by a perusal of the cases alluded to, and the recollection of a case which occurred in my own practice a few months ago. As I did not take notes at the time, I must apologize for the history of my case not appearing in regular detail. The distinguishing symptoms, however, are sufficiently fresh in my recollection for the purport of this communication, should you not think it unworthy insertion in your next number.

Mrs W. a thin delicate woman, about 50 years of age, was seized with general symptoms of pyrexia, as I learned from her friends, a few days before I was sent for. On my first visit, I found

* A case recorded by Mr Tuge, junior, of Plymouth.

found her much prostrated in strength, her skin having febrile heat, the pulse rather below the standard in point of number, yet remarkably full, with a vibrating wirey throb, conveying the idea of inflammatory action. The tongue was white and coated; the bowels rather costive; the ankles were swelled and painful, though not red; the knees were much more swelled, and so painful, that she could not bear the slightest touch without crying out; but the most distressing symptom was an excruciating pain over the left temple, with violent throbbing and a hissing noise, which rendered her deaf on that side. The remarkable prostration of strength, in a frame otherwise very delicate, deterred me from having recourse to general bleeding. I therefore preferred local bleeding, by leeches applied to the painful temple, and a blister applied to the back of the neck. As it was then night, I ordered a sudorific draught to be taken immediately, and a dose of purgative pills to be taken early the next morning; sinapisms were also ordered to the painful joints. The next day she experienced some relief from the remedies applied to the head and temples; but the joints of the inferior extremities continued equally painful, and those of the superior began to take on a similar action, particularly the hands. The opening pills, which had produced their usual effect, were repeated; powders, with potass. nitrat. were ordered three or four times a day, together with antimonials, and the joints affected rubbed with ol. camph. This plan was followed, in a few days, with an alleviation of some of the symptoms, but an aggravation of others; the joints of the inferior extremities grew better, whilst those of the superior became worse. The violence of the pain of the temple, it is true, was somewhat abated; but the hissing noise remained, though I think she was not quite so deaf. Great watchfulness now supervened, with remarkable anxiety at the præcordia, so that I found it necessary to exhibit an opiate combined with an antimonial at night; the powders were changed for a camphorated mixture, to which was added some liq. ammon. acet. given every fourth hour. A little more rest and ease was procured in consequence of this alteration; but now the complaint, which so far may be considered to have been purely rheumatismus acutus, with the addition of the peculiar affection of the left temple, assumed a change of symptoms. The inflammatory action seemed to fly, as it were, from the circumference to the centre. The heart and arterial system became peculiarly affected; their pulsations were slower than natural, but remarkably full and violent, so that the action of the carotids could be seen almost at any distance of the room, and when the hand was applied to the region of the heart, it seemed by its force to be lifted from the part. This violent action

was

was now and then followed by a sudden collapse, consequent faintings, and slight twitchings. It will be expected now that the pain of the temple, and the hissing noise from the force of the carotids and temporal arteries must have been increased. The case assumed a serious aspect. I therefore drew about ℥xvi. of blood from the arm, and the bowels being previously well purged, ordered an anodyne sudorific draught. The next morning the assistance of Dr Woollcombe was requested. The blood appeared somewhat, not particularly, sizy, but certainly the patient was relieved by its extraction. The Doctor's attention was particularly struck by the violent arterial action, and, on turning down the bed-clothes, our patient being very thin, the systole and diastole of the abdominal aorta were evident to the eye. He was equally surprized at the slowness of the pulsation, which at that time was not much above 40 in a minute. Having recommended the left side of the head to be previously shaved above the painful temple, he ordered a blister to be applied there, and prescribed an eight ounce mixture, similar to that I had previously sent, but adding to it ʒi. vin. antim. tart. of which a fourth part was to be taken every sixth hour, with a powder containing xv. gr. jalap, until the bowels were freely opened. A draught, with æther t. lav. c. and t. opii was to be given at night after the operation of the powders, especially should the watchfulness continue. The next morning, the pain of the temple was much relieved by the application of the blister, and the general arterial action was moderated, perhaps by the blood previously drawn, but also, I think, by the purgatives and vin. antim. tart. now prescribed. As the action of the heart and arteries had a little subsided, so their pulsation had risen a little nearer their standard. Indeed it is to be observed, that the pulse gained in velocity what it abated in force, gradually, as the patient recovered, approaching the natural standard. Following this plan of treatment, Mrs W. in the course of a week, was convalescent, every symptom vanishing, together with the deafness, except slight stiffness and occasional shootings in her joints, which in another week or two left her; and I have lately had the pleasure of seeing her in perfect health.

The peculiarities of this case are the extraordinary pain of the left temple, and the remarkable slowness of the pulse. The tendency of inflammation is to accelerate the pulse; it is therefore difficult here to account for its being retarded. The digitalis, so judiciously used in Mr Russell's case, was thought of in this, but the slowness of the pulse, and more especially the extreme debility of the patient, were the reasons why it was not exhibited.

Plymouth-Dock, March 8, 1814.

X.

X.

A Letter addressed to the Managing Committee of the Fever Hospital, in Cork Street, by the Permanent Physicians to that Establishment, on the Treatment of Fever by Blood-letting. To which is prefixed a Letter to the Editors.

SOME time previous to the publication of your Journal for October last, in which we find an analysis of Dr Mills's "Essay on the utility of Blood-letting in Fever," it had been suggested to us, by several of our brethren, that we ought to submit to the different medical reviewers, our printed letter sent herewith, and thus anticipate any erroneous impressions likely to be made by certain positions laid down in Dr Mills's book. A dislike to take any step leading, however remotely, to professional controversy, or personal collision, more than counterbalanced the weighty authority of our advisers; and, under the influence of similar feelings we are, even now, slow to enter on the subject. Yet it is proper that this backwardness should, at length, give way to a sense of duty, and a love of truth.

Sedulously avoiding all discussion as to the general merits of Dr Mills's treatise, we are bound to reassert the charge of error and misstatement, as regards some of his most important deductions and facts. In speaking thus strongly, we would by no means be understood to impeach the author's motives, or to impute to him any portion of the "*mala fides*." Unskilled, indeed, must they be in the crafts of authorship, and of physic, who cannot form some estimate how far the artist may be hurried by a fondness for discovery, or seduced by the animating desire of celebrity and wealth. It is not for us in this place to inquire how Dr Mills has suffered himself to be drawn into those unfounded assertions by which we have been forced into a most minute and laborious investigation of the matters of fact; but it has become our duty and our right to declare to the medical public, through the medium of your Journal, the result of that investigation. For its accuracy, almost to a *fractional* part, we once again pledge ourselves.

To your judgment we, of course, submit the *mode* to be adopted by you. May we suggest, as the most plain, candid, and convincing, a reprint, in your pages, of our "Letter to the Managing Committee of the Fever Hospital." We have the honour to be, &c.

FRANCIS BARKER,
WILLIAM STOKER,
GEO. HAGAN,
} Permanent Physicians.

R. GAMBLE, late one of the permanent physicians to the House of Recovery and Fever-Hospital, Cork Street, Dublin.

Dublin, March 1st 1814.

To

To the Managing Committee, &c. &c. &c.

GENTLEMEN,—The following statements, intended for immediate publication, are most appropriately addressed to you. You are most competent to judge of their force, and to attest their truth, as the documents on which they rest have been formed under your inspection, and are in your possession.

In the observations about to be submitted to you, we disclaim all personal feeling, and shall, as much as possible, avoid all personal remark; nor should we have deemed it incumbent on us thus to solicit your attention, had not assertions been recently and publicly made, tending to affect the interests of this institution, as well as our professional character.

You need scarcely be reminded, gentlemen, that, in consequence of the then unexampled increase of the number of applicants to the fever hospital, you did, on the 21st of June 1810, appoint Dr Thomas Mills to assist as a temporary physician, in which capacity he acted during four months. Similar circumstances induced you again to call in his services in the month of January 1811, and in April 1812, which were continued during eight months. Of his medical practice he has published the result, in a work professing to demonstrate the utility of general blood-letting as a remedy in fever.

On examining his book you will find that Dr Mills has stated (on your authority) the gross number of deaths in the hospital during seven years and about eight months, to amount to about one-twelfth of the number of patients received, or the proportion of deaths to recoveries as one to eleven.*

He has also stated, that, among the patients placed under his care and treated by blood-letting, at one period the average proportion of deaths to recoveries was less than one to twenty-three,† at another period less than one to twenty-eight,‡ and consequently the total average proportion of deaths to recoveries, as one to twenty-five and a half nearly. He has deduced the following inferences, which we quote in his own words: "The following advantages are to be derived from blood-letting, viz. a diminution of the mortality of fever; the preservation of the strength of the patient by the more speedy and effectual removal of the disease." (see page 222 of his book.)

A reader of these assertions, and of the numerous cases which he has detailed, not having the leisure or opportunity requisite for minute investigation, might be led to conclude that Dr Mills has been much more successful in the treatment of fever than the

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* See Dr Mills's Essay, p. 2. † See his Book, p. 220. ‡ See p. 221.

the other physicians of the hospital; that he has lost by death but a twenty-fifth of his fever patients, whilst his colleagues have lost of theirs a twelfth; and that his success has therefore exceeded that of the other physicians in more than a twofold proportion. This opinion once adopted, would necessarily be followed by the belief that the disease was also rendered shorter, and the patient's strength less impaired by his than by other modes of practice. The obvious inference then from the whole would be, that Dr Mills's medical colleagues have been most strangely and culpably negligent in witnessing, without adopting, his plan of treatment.

Thus exposed to imputations which, if established, must render doubtful our competency to discharge the duties of physician in this great public institution, we feel ourselves peculiarly called on to investigate and lay before you the state of facts; and we regret, for the sake of the public, to announce as the result of our inquiry, that Dr Mills's mode of treatment has not been attended with more success than ours, nor produced more frequent or more speedy recovery from fever.

To prove this assertion, we would first observe, that the registry of patients, now extended to more than 12700 cases, shews, that recovery is much more frequent in one year than in another. Thus, in the year 1810, of the patients admitted there died one-tenth, but, in the year 1809,* one-thirteenth only. The proportion of deaths to recoveries varies also during different months of the same year. Thus, in the months of May, June, and July 1812, the proportion of fatal cases was one-fourteenth of the whole, but in the months of October, November, and December of the same year, the proportion of fatal cases was one-thirteenth. Similar remarks apply to the duration of the disease, which is variable in different years and seasons.

Hence it follows, that, to deduce legitimately that fevers have been rendered less fatal, or recovery accelerated by the system of blood-letting, as practised by Dr Mills, the comparison should have been instituted between the success of that and other modes of treatment, during the *same* times; and that Dr Mills should have compared the result of his practice either with that of his colleagues, or, if this mode had appeared to him invidious, with that of other methods of treatment as practised by himself, during the *same times*, instead of adopting statements which, even admitting them to be correct, are altogether inconclusive. This deficiency the physicians have undertaken to supply, and to the

* The year preceding that of Dr Mills's attendance.

the motives already assigned for this procedure they would add, their desire of investigating the merits of blood-letting as a general remedy in fever, considering this an important medical question, capable of some elucidation from the practice lately adopted at the fever-hospital

With these views, we proceed to explain the annexed tables. It should be observed, that Dr Mills practised blood-letting with a very large majority of his patients; and it has been asserted, we believe correctly, that, at times, nearly all his patients were blooded. By the other physicians, this remedy was employed but rarely; consequently the numerical statements contained in these tables will afford a proper estimate of the value of repeated general blood letting, compared with other modes of practice. In these tables, *all* the patients treated by Dr Mills and by his colleagues are given, together with the result of their cases during the different times of his attendance.

In column I. are given the different months of Dr Mills's attendance. In column II. all the patients male or female treated by him, together with the event, whether successful or otherwise, and on the same lines in col. III. a similar statement of all the patients treated by the other physicians during the same months. To save the trouble of calculation, the proportion of deaths to recoveries in each month, among the patients of different sexes treated by Dr Mills and the other physicians, is given in column IV.; underneath the total result is given; and thus the reader can by inspection determine the merits of the different modes of practice, as it affected the proportional number of deaths during the whole or different periods of Dr Mills's attendance. From this table it also becomes evident, that the proportion of deaths to recoveries is variable at different seasons, and this is the case when there is little variation in the mode of treatment; thus rendering doubtful any inferences deduced from varied methods of practice, when not employed at the *same* times, and demonstrating, that the system of comparison adopted by Dr Mills is altogether fallacious.

It appears, as the final result, that, among the patients treated by blood-letting, the proportion of deaths to recoveries was as 1 to $11\frac{43}{53}$ and among those treated according to the more ordinary methods as 1 to $12\frac{51}{110}$, a proportion differing from the former in no small degree, and justifying the conclusion, that the treatment of fever by small and repeated general detractions of blood is either of little efficacy, or injurious.

Thus we reply to the assertion "that the mortality in fever is diminished by blood letting." To determine in what degree recovery has been accelerated by the practice, we have had re-

course to the hospital records, which to us appear well adapted for this purpose. To understand how these can be applied to ascertaining the time of convalescence, it should be observed, that the number of days which each patient has remained in the sick wards, the convalescent wards, and in the whole hospital, is regularly entered in the hospital registry; consequently the average number of days during which any number of patients has remained in these wards, during any required time, is a simple arithmetical question. This we have ascertained for the patients of Dr Mills and his colleagues during the different months of his attendance, and have given the results in the second table.

The conclusion which follows from the latter inquiry is, that Dr Mills's patients have remained in the hospital, on an average, 16¼ days, the patients of the other physicians 17⅓ days; a difference very slight, and, though apparently in favour of blood-letting, yet most probably arising, not from this practice, but from Dr Mills having had under his care a much larger proportion of male patients than the other physicians, * and these patients, from obvious reasons, are less disposed than the females to remain unnecessarily in the hospital after their recovery. Hence, the physicians infer that no sufficient evidence exists to justify Dr Mills in the conclusion that recovery is accelerated, or the patients' strength preserved by blood-letting; and it does not, from any other inquiry, appear that his were less liable to relapse than any other patients, and in many instances they were observed to remain in the hospital in such states of debility as are usually consequent to the repeated detractions of blood.

It must add to the weight of the above conclusions to observe, that the data on which they depend have been furnished from the most authentic sources, namely, the diet tables, in which each physician enters daily the patients' diet, and at the same time registers the date of admission and dismissal, the time of remaining in the different wards, along with the final event of each case; and the different parts of the system being so contrived as to check each other, an error in the tables can scarcely exist without detection.

From the whole of the facts thus laid before you, we feel ourselves authorized to deduce the following inferences:

First, That, including all the patients placed under the care of Dr Mills, at no period did he lose so small a proportion as a twenty-fifth.

Secondly,

* See the Table.

Secondly, That the proportion of deaths to recoveries was in no inconsiderable proportion GREATER among the patients of Dr Mills than among those of his colleagues, and was nearly that of one to eleven * among his patients during his attendance.

Thirdly, That no proof exists that recovery was rendered more speedy or more perfect by blood-letting. †

Having thus stated the facts as far as may be necessary for self-defence, we do not think we should proceed further; any comment on a system of practice, which by an induction formed from 2240 cases, is proved to exert no influence in rendering the disease less fatal or less protracted, would be altogether superfluous, as well as inconsistent with the object of this address.

To the above mentioned conclusions of Dr Mills, and to these alone, we have endeavoured to direct your attention. Disagreeable as the necessity may be which has compelled us to unite in this statement, the magnitude of the mischief attendant on misrepresentation or error in medical inquiries, more than outweighs the dislike we must naturally feel to become the instruments of detecting them; and we owe it to the public, and to the profession, not to permit silence on our part to be interpreted into an admission of the truth of assertions so boldly put forward, and so pertinaciously maintained.

FRANCIS BARKER.
WILLIAM STOKER.
GEORGE HAGAN.
RICHARD GAMBLE.

July 1813.

TABLE

* The large proportion of male patients placed under Dr Mills's care, amongst whom fever is generally more fatal than among females, will not serve to account for the increased proportion of deaths among his patients, as reference to the table will render evident.

† It should be observed, that Dr Mills's patients had the benefit of the general and perhaps most useful agents employed in the treatment of Fever, namely, cool and fresh air, cleanliness, diluents, purgative medicines, and occasionally blisters and topical blood-letting.

TABLE I.

I.	II.						III.						IV.					
Dates of Dr Mills's attendance extracted from diet tables.	*Of Dr Mills's Patients.*						*Of the other Physicians' Patients.*						*Proportion of Deaths to Recoveries.*					
	Dismissed including Died.			Died.			Dismissed including Died.			Died.			Dr Mills's Patients.			Other Physicians' Patients.		
* DATES.	Males.	Fem.	Total.	Males.	Fem.	Total.	Males.	Fem.	Total.	Males.	Fem.	Total.	Males. Death.Rec.	Females. Death.Rec.	Total. Death.Rec.	Males. Death Rec.	Females. Death.Rec.	Total. Death.Rec.
1810—From July 14, to 28,	22	38	60	9	3	12	44	37	81	3	1	4	1 to $1\frac{4}{9}$	1 to $11\frac{2}{3}$	1 to 4	1 to $15\frac{2}{3}$	1 to 86	1 to $19\frac{1}{4}$
From July 28, to Aug. 25,	32	26	58	3	3	6	42	62	104	4	5	9	1 to $9\frac{2}{3}$	1 to $\cdot\frac{3}{3}$	1 to $8\frac{1}{3}$	1 to $9\frac{1}{2}$	1 to $11\frac{2}{5}$	1 to $10\frac{2}{3}$
From Aug. 25, to Sept. 29,	15	56	71	0	3	3	80	57	137	4	5	9	0 to 15	1 to $17\frac{1}{3}$	1 to $22\frac{1}{3}$	1 to 19	1 to $10\frac{2}{5}$	1 to $14\frac{3}{5}$
From Sept. 29 to Oct. 20,	4	6	10	0	0	0	44	45	89	2	3	5	0 to 4	0 to 6	0 to 10	1 to 21	1 to 14	1 to $16\frac{4}{5}$
1811—From Jan. 1, to 15,	22	0	22	4	0	4	0	14	14	0	4	4	1 to $4\frac{1}{2}$		1 to $4\frac{1}{2}$		1 to $2\frac{1}{2}$	1 to $2\frac{1}{2}$
1812—From May 15, to 30,	9	0	9	2	0	2	67	39	106	8	1	9	1 to $3\frac{1}{2}$		1 to $3\frac{1}{2}$	1 to $7\frac{3}{6}$	1 to 58	1 to $10\frac{1}{9}$
From May 30, to June 27,	25	0	25	1	0	1	34	83	117	2	7	9	1 to 24		1 to 24	1 to 16	1 to $10\frac{6}{7}$	1 to 12
From June 27, to July 25,	70	0	70	3	0	3	18	124	142	1	12	13	1 to $22\frac{1}{3}$		1 to $22\frac{3}{3}$	1 to 17	1 to $9\frac{1}{3}$	1 to $9\frac{12}{13}$
From July 25, to Aug. 29,	70	31	101	6	0	6	73	107	180	6	3	9	1 to $10\frac{5}{3}$	0 to 31	1 to $15\frac{5}{6}$	1 to $11\frac{1}{6}$	1 to $54\frac{3}{3}$	1 to 19
From Aug 29, to Sept. 26,	24	19	43	2	2	4	54	93	147	2	3	5	1 to 11	1 to $8\frac{1}{2}$	1 to $9\frac{1}{4}$	1 to 26	1 to 50	1 to $28\frac{2}{5}$
From Sept. 26, to Oct. 31,	58	21	79	3	2	5	48	148	196	7	9	16	1 to $18\frac{1}{3}$	1 to $9\frac{1}{2}$	1 to $14\frac{3}{5}$	1 to $5\frac{6}{7}$	1 to $15\frac{4}{6}$	1 to $11\frac{1}{4}$
From Oct. 31, to Nov. 28,	83	0	83	5	0	5	11	105	116	1	6	7	1 to $15\frac{4}{5}$		1 to $15\frac{4}{5}$	1 to 10	1 to $16\frac{1}{2}$	1 to $15\frac{4}{7}$
From Nov. 28, to Dec. 31,	78	0	78	4	0	4	12	90	102	2	9	11	1 to $18\frac{1}{2}$		1 to $18\frac{1}{2}$	1 to 5	1 to 9	1 to $8\frac{3}{11}$
Tot during the above periods	512	197	709	42	13	55	527	1004	1531	42	68	110	1 to $11\frac{4}{12}$	1 to $14\frac{3}{13}$	1 to $11\frac{49}{55}$	1 to $11\frac{3}{42}$	1 to $15\frac{52}{68}$	1 to $12\frac{10}{110}$

* Dr Mills commenced his attendance at the Hospital on the 21st of June 1810; the above table commences from the beginning of the week ending July 14th (or from July 7th.) This omission of a fortnight arises from the loss of one of the diet tables, and cannot materially affect the total result.

TABLE II.

Dates.	Of Dr Mills's Patients. Average No. of days in different Wards, viz.			Of the other Physicians' Pts. Average No. of days in different Wards, viz.		
	In Fever Wards.	In Convales. Wards.	In whole Hospital.	In Fever Wards.	In Convales. Wards.	In whole Hospital.
	Days.	Days.	Days.	Days.	Days.	Days.
1810—July	$8\frac{54}{61}$	$4\frac{7}{61}$	$12\frac{57}{61}$	$13\frac{56}{99}$	$5\frac{78}{99}$	$19\frac{36}{99}$
August	$11\frac{54}{68}$	$4\frac{60}{68}$	$16\frac{47}{68}$	$10\frac{79}{119}$	$4\frac{79}{119}$	$15\frac{36}{119}$
September	$11\frac{51}{57}$	$2\frac{36}{57}$	$14\frac{23}{57}$	$8\frac{86}{126}$	$4\frac{43}{126}$	$13\frac{83}{126}$
1811—Jan.	$7\frac{6}{10}$	$4\frac{5}{10}$	$12\frac{5}{10}$	$13\frac{4}{33}$	$8\frac{6}{33}$	$21\frac{10}{33}$
1812—May and June	$12\frac{1}{19}$	$3\frac{10}{19}$	$15\frac{11}{19}$	$10\frac{96}{201}$	$4\frac{183}{294}$	$15\frac{82}{291}$
July	$11\frac{26}{88}$	$4\frac{15}{88}$	$16\frac{13}{88}$	$9\frac{180}{230}$	$5\frac{36}{230}$	$14\frac{226}{240}$
August	$12\frac{2}{69}$	$6\frac{59}{69}$	$18\frac{61}{69}$	$10\frac{8}{130}$	$5\frac{50}{130}$	$15\frac{58}{130}$
September	$13\frac{11}{33}$	$6\frac{10}{33}$	$20\frac{6}{33}$	$10\frac{3}{164}$	$5\frac{98}{164}$	$15\frac{101}{164}$
October	$12\frac{20}{77}$	$5\frac{47}{77}$	$17\frac{67}{77}$	$10\frac{153}{163}$	$6\frac{157}{163}$	$17\frac{147}{163}$
November	$11\frac{1}{11}$	$3\frac{60}{11}$	$14\frac{61}{11}$	$15\frac{90}{111}$	$5\frac{10}{111}$	$20\frac{100}{111}$
December	$12\frac{55}{76}$	$3\frac{69}{76}$	$16\frac{48}{76}$	$14\frac{82}{113}$	$6\frac{30}{113}$	$20\frac{102}{113}$
Total	$11\frac{164}{369}$	$4\frac{187}{369}$	$16\frac{302}{369}$	$11\frac{1205}{1304}$	$5\frac{645}{1304}$	$17\frac{1041}{1304}$

XI.

Pathological and Practical Observations.

(Continued from p. 206.)

THE inadequacy of the causes to which acute diseases are constantly referred, to account for their production, has often excited distrust respecting their effect in inducing the diseases attributed to them. But the inference drawn from such doubts, has pointed rather to some secret and unexplained operation of an unknown exciting cause, than to a predisposition, arising from natural and familiar occurrences, and only acted on by a casual excitement. The effect of such excitement, in hastening the occurrence of acute disease, and even in directing it towards a particular organ, there can be no doubt of; but I am equally certain that the violence of disease is in almost every instance dependent,

pendent, not on the exciting cause, but on the state of preparation in which the body is found at the time of its operation.

There are still further dangers, however, resulting from the practice now reprobated, which require to be exposed, and which, though less imminent than those which attend acute inflammation, are perhaps more certain and more to be dreaded. For, when active inflammation arises, free evacuations are generally employed; and, if the patient survives, he very frequently recovers with an amended constitution, and is surprised to find himself at once relieved from the load of undefinable disease which had previously oppressed him. And accordingly we find the effect of acute diseases in relieving the system from nervous affections of long standing, frequently recorded, and generally allowed. The explanation has been, that the elements of disease, so long lurking in the habit, were by the febrile action elaborated and expelled, and I believe it: but I cannot agree that these elements were of any specific nature, or other than the accumulations of redundant nutriment, and of excrementitious matter undischarged. Partly by the increased actions of the system, which constitute a state of fever, but principally by the evacuations employed to relieve this, these excesses are removed,—the system is relieved from an oppressive load,—its functions are again actively and healthfully performed, and the whole constitution is invigorated and improved. If the danger, therefore, of acute inflammation is great, the benefit oftentimes resulting from it is great also. Not so the more slow and insidious attacks of inactive inflammation, which so constantly take place in languid or enervated habits, which alters the structure, and impairs the functions of the organ it assails, and finally destroys life, after a long course of suffering, by the manifold ills which arise from it.

Yet the remedies for this state, if duly appreciated, and timely employed, are, I believe, equally certain and effectual as those which so often relieve acute inflammation. And my conviction is, that moderate blood-letting, if resorted to when the evidences of overloaded circulation first appear, or even when those of increased action in the vessels of any viscus first present themselves, might be so employed, as to avert altogether the occurrence of organic lesion, by correcting at its source the error which gives rise thereto. Unfortunately, however, the nature of the habits most prone to this species of disease, leads to a belief of constitutional debility, to which much of the incipient disease is referred. The practice is regulated accordingly; some amelioration attends its early employment; and this first and radical error gives rise to such complexity in the after-course of the

the disease, that there is little hope of any simple exposition of symptoms being afterwards arrived at, or a judicious course of treatment being steadily pursued. In such cases, the delicacy of constitution may seem to afford some plea from abstaining from the use of remedies, which are generally conceived debilitating, and for resorting to others which are as generally believed to strengthen and invigorate. But, if rightly understood, this very delicacy of frame affords an argument, not against the employment of blood-letting, but against hazarding any thing by undue delay, inasmuch as the evacuation of blood, being essential to the safe and speedy cure of the disease, and the constitution being unsuited to undergoing much of this, an early employment is likely to be more speedily effectual, and to be better borne, while the strength is yet unbroken by disease. Surely blood-letting, so employed, tends to preserve strength, rather than exhaust it, even admitting that its direct effect is necessarily and inevitably debilitating; for, by early employment, a less copious depletion suffices, while it arrests the progress of disease, from which greater and much more fatal debility would otherwise ensue.

That delicacy of constitution will require depletion to be more sparingly employed, is a truth which scarcely affects the question at issue, for in such habits less will be necessary. In every instance a given effect is sought for; in the more delicate, this is obtained by a less copious depletion than is required in the vigorous and robust; in all there are certain limits to bound its employment; and within these it will, I believe, be found, that there are few habits that will not perfectly well sustain it. These limits should certainly be well defined and clearly understood, by all who make much use of blood-letting in the cure of diseases, and, under the guidance of that pathology, which I have been endeavouring to support, it will not be very difficult, I hope, to determine them. If blood is drawn in the incipient stages of disease, the increase of tone in the pulse consequent to the evacuation will sufficiently mark its safety and propriety. A renewal of the oppressed and irregular pulse will naturally call for a repetition of venesection; and, finally, the removal of the disease dependent on plethora, and the restoration of the pulse, to soft, equal and regular pulsations, will sufficiently evince when the remedy may be discontinued. On the contrary, when employed for subduing inflammation, the pulse will afford an equally certain guide for regulating its frequency and amount. In both cases there can be no pretence for carrying its use beyond the restoration of the pulse to the condition of health, and within the limits here pointed out, which a knowledge of the principles necessary

to

to be kept in view will prevent our misapprehending, the employment of blood-letting must ever be both safe and salutary. My more immediate business, however, in this essay, is with the stages of disease antecedent to inflammation; the consideration of which latter, therefore, I wish to wave as much as possible, or only to advert to for the purpose of illustration.

The efficacy of blood-letting in relieving the train of symptoms belonging to these antecedent stages, I have too often witnessed to doubt its reality; and the cessation of rigors, the restoration of warmth to the extremities, and removal of all the symptoms denoting irregular circulation after its employment, sufficiently manifest the real nature of the causes whence such symptoms proceed. It may not be improper, in this place, to enter a little more largely into a consideration of the principles which should guide the use of the lancet in the complaints now treated of; and, perhaps, this is done by contrasting it with that employment of blood-letting which inflammatory affections require. When blood is drawn to relieve a state of increased arterial action, or local inflammation, the general directions are to take it from a large orifice, and in a full stream, so as to affect the moving powers by the suddenness of the depletion, as well as by its amount. And the principle on which such rule is founded, is perfectly just, as there is a necessity not only for diminishing the plethora, but also for moderating, as speedily as possible, the arterial action induced by it, which might otherwise go on to disorganize some important structure, before the plethora could be sufficiently corrected or removed. And if this purpose was steadily borne in mind, and the importance of attending to it clearly understood, the practice in acute inflammation would be more decisive, and more effectual. For, I am well assured, that one blood-letting, carried to the extent of inducing syncope, makes greater impression on a state of inflammation, whether local or constitutional, than several which do not produce this effect, even though individually amounting to what may be considered a full blood-letting.

To what amount this term should be applied, practitioners, I believe, are not well agreed; but, if we are to abide by the statements of our most celebrated writers, both systematic and practical, I must consider it as rated much too low. Cullen, in treating of pneumonia, says, that in an adult male of tolerable strength, a pound avoirdupois is a full blood-letting, and any quantity over twenty ounces a large one. If evacuations, however, are limited by either of these quantities in cases of active inflammation, I have no doubt but that many lives must be sacrificed, which a bolder practice would preserve. In short, the end of blood-letting

blood-letting so employed is to reduce arterial action; and, until some impression is made thereon, the flow of blood should not be stopped, even though double the amount of either of these quantities be taken. I have repeatedly drawn from thirty to forty ounces at a single blood-letting, with most salutary effect, and am satisfied, have succeeded in such cases in subduing disease with less eventual loss of blood than would have been requisite, had smaller blood-lettings been trusted to, while the danger of protracted disease has been more effectually averted. I even know an instance which occurred in this town, where above seventy ounces were taken at once, to relieve violent determination to the head, with threatenings of apoplexy; and the event fully justified the practice, for the very imminent danger to life which was manifested, was by this means removed, while the patient was so little exhausted by the unusually copious depletion, that he actually rode from this town to Bristol, on some particular business, the next day. In my own person I have lost sixty ounces within a few hours, in two blood-lettings, and am well assured that I owe my life to the treatment. In short, the freedom with which blood-letting may and ought to be employed for the relief of active inflammation, is not understood as generally, or practised as extensively as it ought to be. In time, I trust, the minds of practitioners will be more open to its advantages, and the prejudices which now so generally prevail, cease to oppose a bar to its necessary employment. A case, which may perhaps be deemed an extreme one, has been reported to me within these few weeks, by a judicious medical friend, under whose care it occurred, and deserves to be mentioned, as shewing the extent to which blood letting may be carried with safety and advantage. He was called to a person labouring under enteritis, whose previous attendant had pronounced him too weak to bear blood-letting. Regarding the case in a very different light, my friend proceeded to take blood copiously, convinced, from the violence of the symptoms, that there was no other chance for saving life. He continued the evacuation till the symptoms abated, and again had recourse to it, when the renewal of severe pain called for its repetition. So violent were these renewed attacks, that blood-letting was repeated four times in twenty-four hours, and a quantity of blood thereby taken which weighed six pounds and a half, or one hundred and four ounces; the consequence was, that the disease was subdued, and the patient rapidly recovered.

In the incipient stages of disease, however, it is never necessary to take blood so largely, or so frequently. The purpose is, to abstract gradually a portion of the circulating fluid; and, unless a state of increased action supervenes in the course of treatment,

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this had much better be done without making an impression on the moving powers. Contrary to what is enjoined, therefore, in cases of inflammation, blood should here be drawn rather slowly, and from a moderate orifice. At first small blood-lettings only should be taken, perhaps not exceeding twelve ounces, for at this time faintness is readily induced. After a few times, however, the danger of this ceases, and from sixteen to twenty ounces may be taken, with little sensible effect on the patient's feelings. The repetition is to be regulated by the symptoms which have been laid down, as indicating its employment, and as soon as the oppressed and irregular action of the pulse begins to revive, the necessary relief should be afforded by a repetition of the blood letting.

Moderate blood-lettings, if timely employed, will, for the most part, relieve the early stages of disease, and restore perfect health, without giving rise to any interval of febrile or inflammatory action; or if this state shall have actually commenced, adequate evacuation will subdue it, generally without its manifesting any increase of violence under their employment. And on this subject I have acquired some experience in my own person, so explicit, and so little likely to deceive me, that I must consider it as well worth recording.

About five years ago, I became subject to severe inflammatory attacks, affecting more particularly the head, and amounting indeed nearly to phrenitis. Under such vigorous practice as I have lately been noticing, adapted with promptitude and decision, under the superintendence of my excellent and judicious friend Dr Langworthy of this city, I have experienced in every instance speedy relief. The attacks recurred several times, and at short intervals. Other inflammatory tendencies were also occasionally manifested, for I suffered successively from cynanche tonsillaris, accompanied with high inflammatory fever, from a very active attack of inflammation in the knee-joint, and from severe general rheumatism. These warnings were quite enough to direct my attention both to the causes of such frequent recurrence of disease, and to the symptoms whereby its approach could be foreseen. Of the former I could not long remain in doubt, for one common character of plethora marked all the attacks I had sustained. This I was not naturally subject to, and it manifestly arose from the indolent and sedentary habits of a city life, the discontinuance of active exercise, and the assumption of a greater portion of nutriment, than, under such diminished exertions, the system could properly dispose of. In accounting for the tendency to cerebral disease, so strongly manifested, and for the frequent determination of blood to the head, I am inclined to attribute somewhat to the influence of crowded rooms,

and

and heated apartments, in which, on my first arrival in this city, I was too frequently immersed, but still more to mental excitement; for almost every severe attack I have sustained, immediately ensued on some occasion of peculiar anxiety, caused either by illness in my own family, or by the danger of some friend committed to my care. These reasonings pointed out the propriety of avoiding as much as possible the causes of disease now enumerated; and though I may not in every instance have adhered as closely as I ought to the course they seem to prescribe, yet have I been very materially benefited even by partially adhering to the regimen they suggest.

With the premonitory symptoms I soon became perfectly well acquainted,—and these, as most connected with my present purpose, I shall endeavour to describe as accurately as I can. Increasing foulness of the tongue has, in general, been the first symptom to mark the approach or commencement of febrile action; and this, I am satisfied, affords one of the most valuable indications which the physician can avail himself of in forming a judgment of diseases. Even when the state of the pulse is equivocal, and when no increased action can be detected therein, if the tongue is loaded, and more especially if it presents that peculiar whiteness of its substance, so remarkable in the onset of active inflammation, blood may always be freely taken, in the full conviction, that, under evacuations, the pulse will soon unequivocally pronounce what the nature of the complaint really is. The changes which take place in the state of the tongue, I believe, are only correspondent to those which are going forward in every other part, in consequence of the increased action of the arterial system. This naturally gives rise to increased secretion; the product of which, accumulating on the tongue, furnishes that morbid matter, so universally present in febrile complaints; and which may so often be noticed, and in considerable quantity, even on the tongues of many who consider themselves in perfect health, especially if examined at morning-time.

Such appearance of the tongue, I have no doubt, requires to be attended to, and should suggest the expediency both of abstinence and evacuations. I do not say that blood-letting is at such times always requisite, although I believe it would always be safe and proper, and certainly the most efficacious means of correcting the diseased actions going forward. Other evacuations, especially if assisted by abstinence, may, however, suffice, and perhaps may be safely trusted to. The principal circumstance to attend to, is, that this diseased aspect of the tongue arises from a state of arterial action, analagous to inflammation;

that it indicates the commencement of a similar state in all the other arteries of the body; and that, if the existence of this condition is not borne in mind, and if it is not corrected by suitable treatment, a state of general fever or inflammation will too probably supervene.

A similar state of increased secretion to that which the tongue presents, occurs, I have no doubt, throughout the whole alimentary canal; the connection between which and the tongue has often been remarked by practical writers.

To it is owing, as far as my judgment extends, the next premonitory symptom of approaching fever which I have to notice,—namely, nausea. It has been said, that, at the approach of fever, the stomach refuses to take in food, because its introduction would, under such circumstances, prove injurious. The business of the pathologist is, however, with efficient, not final causes; and it seems of much more consequence to scrutinize the operations of nature in thus cutting off the supply of food, than to confine our observations to the benefits which result from diminished nutrition. And this, I think, we may do with much satisfaction, by aid of that simple pathology, which it is the purpose of this essay to inculcate. Increased actions of arteries necessarily induce increased secretions; these, if continued, soon become different from the healthy ones; or, in other words, morbid; a disturbance in the healthy functions cannot but ensue; and this, in the stomach, speedily manifests itself by anorexia. In the early stages of disease, this, however, is not constant. With me it was only perceptible at evening-time, and not till several hours had elapsed after taking my principal meal.

In addition to these symptoms, others, indicative of increasing arterial action, were soon superadded. The skin became hot, the mouth dry, pains of head increased, and the pulse, on examination, was found either oppressed and irregular, or hard, strong, and frequent. Blood, taken at this time, shewed generally some appearance of buff; and one copious depletion, aided by purging and abstinence, was sure to cut short the disease, without subjecting me to an hour's confinement.

As my experience increased, I was able to anticipate still further the progress of disease, and to meet it by suitable treatment, before inflammatory action had advanced to the period just stated. Foulness of tongue was still the first symptom to give warning, and this was oftentimes removed by means of a reduced diet, aided by the daily use of small doses of Epsom salt, without any further disease ensuing. Uneasy feelings of head, however, did occasionally set in, notwithstanding the purging; and

and these have, in every instance, preserved such a uniformity of character, as is worth describing.

At first, an uneasy feeling began to be perceived in the vertex, slight, and not amounting to pain. This was succeeded by a sense of coldness, as of water poured on the part. When examined by the hand, however, it has always at such times been found preternaturally hot, the heated part being exactly circumscribed, and occupying an oval space, including the whole of the vertex. The difference of temperature between the vertex at such time, and the adjoining occiput, has been great and remarkable, and has been ascertained by such a variety of hands, as precludes all possibility of deception. At such times, one full blood-letting has always given instant relief, the uneasiness of head departing while the blood yet flowed from the arm. I have lately, under similar circumstances, lost 26 ounces of blood at one blood-letting, without feeling weakened, or being obliged to forego my daily occupations.

If postponed for any length of time, nausea occurred; the eyeballs began to ache, and were pained by pressure; the lids did not play freely over their surface, but, in doing so, excited uneasy feelings, and the pupils became somewhat dilated. It is needless to pursue this detail further. Suffice it to say, that, by early recourse to blood-letting and abstinence, on the first intimation of increasing plethora, I have now, for above two years, repeatedly succeeded by measures, speedy and effectual in their operation, in averting attacks of disease, which I have every reason, from the former experience of my own case, to think, would otherwise have been both severe and dangerous. And, in similar cases, I would strongly enjoin every practitioner to have speedy recourse to the only means adequate to removing the complaint, as soon as the evidences of local determination appear. For the continuance of such state is never harmless, even though the increased action may not go to the extent of actual inflammation. For, by congestion, or fulness alone, the structure and functions of an organ may be very materially impaired. In time the over distended vessels become permanently dilated, and, in such an organ as the brain, even the pressure thus made on the interstitial substance cannot but be injurious.

Nor is this an imaginary case; for I have now a patient who has suffered under determination to the head for many years. Inflammation did not take place, and the strength of the arteries enabled them to resist the violent impetus with which the blood is sent through the carotids, so as to preserve from apoplexy. But the capacity of the arteries has been gradually increasing by the dilatation of their coats; and the diameter of the carotids

cannot,

cannot, at this moment, be less than from half to five-eighths of an inch. This case originated in great mental distress, arising from abused confidence, and derangement of circumstances, and the inordinate action of the arteries, which exceeds any I ever before met with, is only kept under by frequent blood-letting, and extreme abstinence. There are two further circumstances in this case which I think it worth while to notice, namely, that the pulse is of that peculiar character which impresses the finger with a sensation similar to that induced by the motion of a water-hammer, and which, I believe, is often found the precursor of hæmorrhage; and that the blood drawn has oftentimes, in addition to the thick and firm buff on the surface of the crassamentum, displayed in the centre of this, when cut through, an irregular mass of clear and colourless coagulated lymph, resembling a lump of melted glass.

This man is near 65 years of age; appears ruddy and like a person in perfect health; takes daily exercise on horseback; but suffers continually a variety of distresses which are all clearly referable to a state of fulness and increased action in the vessels of the brain. A further ill consequence of delaying the employment of blood-letting, long after the evidences of plethoric and local determination appear, is, that the relief afterwards afforded is neither so speedy, certain, or effectual, as when this valuable remedy is timely employed. And, in habits liable to local determination, and which may have frequently suffered therefrom, blood should be taken, purgatives administered, and nutritive diet withheld, immediately on the first appearance of febrile symptoms; and the antiphlogistic regimen continued, untill they shall have disappeared. Still better, however, will it be, if this regimen is resorted to when the evidences of plethora first begin to manifest themselves; and I have no doubt whatever but that this state may be always detected by a due attention to the phenomena which indicate it.

The value and efficacy of preventive treatment, regulated by the principles which it is the great purpose of this essay to inculcate, cannot be better illustrated than by the following case, which I have been favoured with by the same judicious practitioner who attended the case of enteritis lately mentioned. It is so expressive, and affords so much valuable information, that I am sure I shall be readily excused for introducing it in this place.

A lady, 60 years of age, had, for the last 20, been subject to gout. For four or five years it presented no unusual appearances; It then became severe, so as in time to deprive her of the use of her right shoulder, elbow, and forefinger, the last of which became

came permanently stiffened. Stimulants and mild aperients constituted the only treatment to this period; when she was sent to Bath, for the purpose of bathing, and pumping on the affected joints, and of drinking the waters for the constitutional disease.

The shoulder and elbow were partially benefited by the external application of the water, and relief was given to some stomach complaints by its internal use, but the general disease was in no respect abated. The medical and dietetic treatment pursued was still cordial and stimulant. In the year 1809 this case came before my friend, at which time she was accustomed to have three or four attacks annually, each continuing for months, and leaving but little interval of convalescence. During these latter she could contrive to move about with difficulty, but at all other times was helpless and obliged to be carried. On the 31st of October he first visited her, when labouring under a severe attack. By free purging this abated, and, on the 7th of November, she was able to dispense with medical attendance, which she did not again require till the following May.

From this to February 1811, she had several paroxysms, having persisted in the use of cordial medicines and of Bath water. Becoming at length alarmed from the violence and frequency of her attacks, she now consented to do as her medical friend had long urged and desired. She was bled several times; was freely purged; had saline medicines; and was enjoined an abstemious diet. By the end of March this attack was thoroughly removed; and, from that time to the present, she has continued free from any violent paroxysm.

Several threatenings of attack were manifested, and two slight ones actually occurred, each of which was distinctly referable to an undue indulgence of appetite in the use of animal food. Aware from experience of the premonitory symptoms which used to usher in the gouty paroxysm,—she had recourse, on the first appearance of such, to her medical friend, in whom her fullest confidence was at length reposed; and who, by purging or bloodletting, as the case seemed to require, had the satisfaction of repeatedly removing the symptoms, without any attack supervening.

While the disease was thus repressed, her general health underwent great and remarkable amendment, and her powers in every respect materially improved. Instead of being carried to her carriage, she can now walk from her own house to the centre of this city, a distance of more than a mile, and on ground mostly ascending.

Nothing can be more decisive, I think, than this case, both in shewing the efficacy of proper treatment, in averting gout, and

in evincing the safety with which it may be adopted, even under unfavourable circumstances. For in this case were combined almost every circumstance which could tend to deter from his purpose a practitioner less thoroughly satisfied of the propriety of his measures.

The patient was in advanced life; had long been a martyr to the complaint; had suffered much disorganization of parts; had her system habituated to a stimulant regimen; and her stomach so relaxed and impaired, that almost every kind of food disagreed, while the general disease was increasing both in frequency and violence.

If a case so circumstanced admitted of such perfect relief as was actually attained, it is difficult to conceive any to which a regulated employment of similar treatment would not be applicable.

At all events, however doubts might exist respecting its adoption with the exhausted and decrepid objects, who, from long suffering under the complaint, have had their constitutions broken, and their vital powers enfeebled, none can rationally be entertained on the propriety of employing it with the young and robust, nor in any case where the great powers of the constitution are yet entire. An instance of its safe and effectual application in such a one may be no unsuitable sequel to the present discussion; and I shall adduce one, which, in addition to this evidence, affords also a demonstration of that general inflammatory state of the constitution which precedes a gouty paroxysm, by attention to which its approach may, I believe, be always foretold.

A friend of mine, about 40 years old, and of full habit, had been subject to gout for several years. He arrived in this country, in the course of last summer, from America, where he had suffered several attacks of the disease. The treatment he experienced under them I cannot specify, but in the intervals he was enjoined to take a pint of wine daily, in consequence of his gouty habit. This he acquiesced in, from unwillingness to put his own judgment in competition with that of his physician, although he disliked the remedy, and thought himself always worse, both for wine and full living. He called on me in autumn last, to know if he should drink the Bath waters, as recommended by his American physician; at the same time, stating himself to be particularly well, and free from complaint. I found his pulse full, strong, and nearly 100, and his tongue whiter than it ought to be. I consequently discountenanced any trial of Bath water, and gave my opinion that he stood much more in need of blood-letting and evacuations. As he professed himself, however,

ever, to be so very well, I did not conceive myself warranted in pressing this, though satisfied that his habit approached much too nearly to a state of inflammation. This conversation took place on Wednesday the 14th of October; and on Thursday he was attacked with gout in his right knee. On Friday a second attack took place in the left foot, and on Saturday morning I was sent for. I found his skin hot; tongue white; and pulse 106, full and strong. Sixteen ounces of blood were drawn, after which the pulse fell to 84. A full dose of a purgative, consisting of extract of colocynth, calomel, and antimonial powder, was given at bed-time, and a solution of Epsom salt the following morning.

On Sunday he was better in every respect, but the pulse was again up to 96. It evinced clearly a necessity for further blood-letting, but as the purgative was then operating, and the local disease abating, I was satisfied to order the venesection to be repeated the following morning. It was not in time, however, to avert a third attack, which took place during the night, in the right elbow. Next morning I found the pulse as much reduced as if blood-letting had been practised, although it did not take place, in consequence of the state of the arm. Whether this effect was owing to a relief to the general circulation being afforded by the formation of the local disease which had occurred in the night; or, whether it proceeded from a degree of syncope and nausea which were experienced at first rising; or, whether they all were dependent on some common cause, I shall not pretend to determine. At this time the knee was nearly well, and the foot much better; and as the tendency to syncope had not entirely gone off, and he felt reluctant to have both arms incapacitated, he expressed a wish that the blood-letting might be deferred till the gouty arm got better.

On Tuesday the foot was almost well, and the elbow getting better. The pulse, however, was again at 96, full and oppressed, which determined me against incurring any further risk, and on having recourse once more to the lancet.

About fourteen ounces were taken, which induced some faintnesss. On Wednesday the pulse was 84, and there was no renewal of gout. Purgatives, salines, and low living were the only means employed from thenceforward, and from this period he rapidly recovered. On the 26th he walked to my house, and could have done so some days earlier, had the weather permitted. He expressed himself much gratified at the treatment he had undergone, as confirming the correctness of those views of the disease which he had always entertained, and declared to me that he had never thrown off such an attack, so quickly, so perfectly, or with such unimpaired powers.

The reflections more particularly suggested to my mind by this case, are, that the evidences of constitutional inflammation

which preceded the attack were sufficiently well marked to have warranted the employment of blood-letting, at the period when I first saw him; and that if then practised, to the extent necessary for sufficiently lowering the circulation, with the assistance of purgatives and low diet, the attack might have been altogether averted. And further I conceive, that if, on the Sunday, I had been influenced by the pulse alone, and disregarding both the faintness and the operation of the purgative, had repeated the venesection, the third attack, which took place in the elbow, would not have occurred.

It has been shewn, in the course of this essay, that the enfeebled and oppressed state of circulation, which marks the early stage of plethora, occasionally gives place to a state of increased or inflammatory action of the arterial system, on blood-letting being employed.

In one remarkable disease, in which the characteristics of incipient plethora, with debilitated powers, signally prevail, it is perhaps essential that this should be the case, and that some degree of fever or inflammation should ensue in aid of evacuations, in order to correct the great and general depravation which pervades and oppresses the whole system.

I allude here to diabetes,—a disease interesting to the pathologist, from the uncommonly vitiated state in which the mass of blood, and several secretions, are found to exist, and on which some highly interesting views have of late been brought forward by Dr Watt, now of Glasgow, to whose excellent treatise I conceive the medical profession is deeply indebted, for placing before them, in such a strong point of view, the early stages of constitutional disease,—and for exhibiting to them a boldness of practice in the removal of such, unsupported by precedent or authority, but fully justified by the reasonings that dictated, and the success that attended it. The merits of this work, I apprehend, cannot be set aside by any objections to which his theories may seem liable. In some respects these may be fanciful and gratuitous, but this circumstance can by no means invalidate his facts or his statements, on the truth and accuracy of which I fully rely. From the views of disease which I entertained previous to the perusal of Dr Watt's book, and which I have endeavoured to present in the foregoing pages, it was natural that I should feel disposed to give a ready credence to facts so analogous to much that I had myself witnessed, and to pathological principles so congenial with those that experience had almost insensibly forced upon my adoption. I acknowledge, therefore, that my early impressions were all in favour of the general doctrines which Dr Watt has advanced, though, I trust, my attachment to them was not so bigotted as to blind me to their imperfections, or ren-

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der me insensible to any contradictory evidences that the course of medical practice might present me with.

In one case only of the peculiar disease he treats of, have I had the opportunity of proving the practice he recommends; and the following brief detail of this will, I trust, sufficiently shew, that nothing has therein occurred to shake the faith which I have been so inclined to repose in his truth and correctness.

The case I have to report was a well marked one of diabetes mellitus, and presented every symptom characteristic of the disease. It occurred in a thin spare boy of 17, who, previous to my seeing him, had been ill for three months, with weakness and emaciation progressively increasing. His appetite was excessive; thirst considerable; his tongue moist when I first saw it, though reported as being generally dry. It was clean, and of a dusky red colour, such as is so frequently met with in the latter stages of phthisis. A quantity of viscid mucus seemed to line the mouth and fauces; his bowels were costive; his pulse 76, small, creeping, and irregular; and, if his account was to be credited, he must have been in the habit of making from 16 to 24 pints of urine each day and night.

As he lived some miles from Bath, and was not prepared then to remain, I ordered him an emetic and some purgatives, and desired that he should return as soon as possible. In the meantime, as it was the first case of diabetes I had seen since Mr Watt's book had fallen into my hands, and as I was resolved on giving the evacuant plan of treatment a full trial, I procured the ready assistance of two medical friends, Dr Langworthy and Mr Long, both of this city, in order that the progress of disease, and the effect of medical treatment, should be observed with all possible accuracy.

On the 6th of November 1811, this treatment commenced; and, from thence to the 29th of December, he lost blood to the extent of 209 ounces avoirdupois, the quantity being ascertained by actual weighing.

To detail the occurrences of the interval, would be nearly to recapitulate what Mr Watt has so accurately reported, so exactly did the case correspond in all its leading features with those he has published. After the fourth blood-letting, a smart attack of erisypelas took place in one leg; and, from this period, the blood exhibited a buffy coat, so long as it was deemed expedient to take it at short intervals.

The minute details of this case I feel unwilling to introduce, both from the addition they would make to an essay, already, perhaps, too much extended, and as being unnecessary, from their very exact correspondence with those which Mr Watt's book so satisfactorily affords. To those who are already acquainted with his cases, I trust the following tabular view of the blood-lettings employed,

employed, will afford a tolerably clear conception of the present one.

No.	Date.	Amount of each.	Quality of blood.	Total amount at each period.
1811.				
1. Nov.	6.	℥ xii		12 oz.
2.	7.	xiv		26
3.	9.	xvii		43
4.	11.	xiv		57
5.	15.	xvii	buffy	74
6.	18.	xx	buffy	94
7.	20.	xx	buffy	114
8.	25.	xviii	buffy	132
9.	27.	xviii	buffy	150
10. Dec.	13.	xx		170
11.	20.	xvii		187
12.	29.	xxii	buffy	209

Purgatives were largely used, from the extreme foulness of the bowels, and nearly 200 grains of calomel were taken, without exciting salivation. For the latter periods he had been in the country, as his parents were unwilling to continue at the expence incurred by his boarding in Bath. The effect of the treatment on the urinary secretion was to reduce its quantity considerably, but never entirely to correct its saccharine quality.

The skin underwent great and decided amendment, being gradually restored from a state nearly bordering on general ichthyosis, so hard, dry, and thick were the scales, which almost universally covered it, to one perfectly soft, smooth, and perspirable.

His general health and strength, too, progressively improved, and though, on relinquishing the use of medicine, his diabetes was not cured, he was, nevertheless restored from a state of debility, which rendered him scarcely able to walk into my study, to a degree of bodily vigour which empowered him in the spring to hold his father's plough eight hours a-day for a week together.

This fact alone, is decisive as to the effect of copious depletion, in restoring strength under this peculiar state of constitution, and fully proves the propriety of the practice, notwithstanding its failure, in this instance, to cure the urinary disease. The boy, I understand, is still alive, and able to labour. He was examined by Mr Long in October last, when he appeared to have all his constitutional complaints returning on him, and was exceedingly anxious again to undergo the treatment, which in the preceding year had so signally relieved him. His father, however, from unwillingness, I believe, to incur the expence of his board, could not be brought to consent, so that I was not again applied to, and in all probability the boy must finally fall a victim to the disease.

(To be concluded in our next Number.)

PART II.

CRITICAL ANALYSIS.

I.

An Inquiry into the Probability and Rationality of Mr Hunter's Theory of Life; being the subject of the first two anatomical Lectures delivered before the Royal College of Surgeons of London. By JOHN ABERNETHY, F. R. S., &c. Professor of Anatomy and Surgery to the College. 8vo. pp. 95. Longman and Co. 1814.

MEDICAL and chirurgical science is indebted to Mr Abernethy for so many solid and valuable additions, the result of his own investigation, the product of his own unquestioned talent, that his reputation is not likely to be advanced by any success which he may obtain, in advocating the hypothetical opinions of any other man, however distinguished. Appropriate, therefore, as the discussion of this Hunterian doctrine might be, as introductory to a course of lectures connected with the Museum of John Hunter, and delivered under the same roof, it would have been well if the compliment had been deposited there, or had been left to be cherished in the remembrance of the auditors of these lectures. The easy and somewhat elegant style in which they are written, aided by the animation of delivery, must have been calculated to impress the minds of the listeners favourably both towards the author and the advocate of the doctrine; but we do not conceive that the logic, either of the one or of the other, will stand the test of a deliberate examination, or that the establishment of the hypothesis, if it were made out as satisfactorily as Mr Abernethy maintains it to be, would be of any value to the physiologist or pathologist. Indeed the objection of being worthless is not confined to this, but is applicable to all speculations about the nature of primary causes: these are beyond the reach

reach of our powers; and are therefore improper objects of our labour; we have nothing to do but with their results, which are alone subject to our cognizance. A theorist may, indeed, display some ingenuity in bringing together a number of imperfect analogies, and in magnifying the points of resemblance among them, and thence may fancy that he is approaching some general cause, from which they all spring; but let us examine these analogies in other points, which the partial advocate has kept out of our sight, and let us collect a multitude of other phenomena, which he has omitted to present to us, because no similarity, but all manner of diversity, was to be observed in them; and we shall be satisfied, that these hypotheses are mere fictions of the fancy, as flimsy as the imagery of the poets.

That *electricity is life* is, we apprehend, the hypothesis "the probability and rationality" of which Mr Abernethy has undertaken to defend; and we shall attempt to follow him through some of his trains of reasoning. We were very much baffled in the outset, we must confess, by the extreme want of precision and perspicuity, which is found in the interpretation of some leading terms. First we are told, that by the word *theory*, is meant "a rational expectation of the cause or connexion of an apparently full or sufficient series of facts; by *hypothesis*, a rational conjecture concerning subjects in which the series of facts is obviously incomplete." p. 8. And in the next page we are requested to reflect, "that hypothesis and theory are the natural and inevitable result of thinking; so that if we refuse to allow of any theory, we must prohibit all thought." And we are farther informed, that, "since thinking is inevitable, our chief inquiry should be how we ought to think or theorize."—"Our theories, hypotheses, or opinions, for to me all these words seem to refer to one and the same act of the mind,—&c." p. 12. So that, as Darwin said long ago, in apology for his own hypothetical fancies, we are to understand that "to think is to theorize;" * a vague and useless acceptation of terms, which is altogether at variance with the specific definition with which our author set out. Nor can we accord with him in his estimation either of the proper origin or of the utility of an hypothesis. He would have us invent our hypothesis first, and investigate the facts hereafter; and thinks "it is highly probable that it was his hypothesis respecting life which incited Mr Hunter to inquiries by which he has been able to supply the deficient facts, so as to establish his conjectures, or convert his hypothesis into

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* Preface to Zoonomia.

a theory." p. 13. Surely this is not the true proceeding of philosophy, to enter upon investigation with preconceived opinions, and to multiply observation with the view of confirming them. The distorted conception and representation of facts which a favourite hypothesis is apt to produce, and the falsehood and error which it thus contributes to propagate, might be amply illustrated by a reference to medical records. Nor does it appear to be either very complimentary to Mr Hunter, or very correct, to suppose that he was only led to the long series of valuable observations, with which he enriched physiological and pathological science, by the stimulus of his preconceived hypothesis concerning life. This observation is, indeed, somewhat incompatible with another statement of Mr Abernethy respecting him, when he says, "I know of no opinion of his that was lightly or loosely formed, or that was not logically and cautiously deduced from the facts before him." p. 21.

As a preliminary to the discussion of the hypothesis, Mr Abernethy relates the opinions of several observers relative to the structure of the muscular fibres, respecting which he considers our knowledge as very limited, in consequence of our limited powers of perception. He then proceeds to describe the principal phenomena connected with irritability or muscular motion; namely, the superior strength and tenacity of the living over the dead muscle; the pain and weariness which result from long continued contraction; and the necessity for alternate remission and action, with the exceptions to this principle in some sphincter muscles, and in violent cramps and tonic tetanus. "The rigid contraction of the muscles after death, is the effect of irritability: it is its last act. A considerable force is required to overcome this contraction, or to bend the rigid limbs of the dead body, when it has recently taken place. The force required to effect this, gradually diminishes till the muscles become quite pliant; and then, and not till then, does putrefaction ensue."—p. 30. In other cases, however, as when death is produced by lightning, or large charges of electricity, or by certain kinds of injuries and diseases, Mr Hunter observed that this contraction, and even the coagulation of the blood, is prevented, and putrefaction rapidly commences: whence that physiologist drew the inference, that "the principle of life may in some instances be suddenly removed, or have its power abolished, whilst in general it is lost by degrees." Mr Abernethy then notices the variety of contractility that occurs in some animals, and in some muscles in other animals, the motions of which are very slow, but powerful and permanent. "The American sloth supports its weight for a very long time by fixing its claws into the branches of trees;

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an act which would speedily weary muscles of an ordinary character. The muscles of the legs of birds that roost, seem to have a similar power of permanent contraction." The peculiar distribution of the arteries in the limbs of these tardigrade animals, as demonstrated by Mr Carlisle, and the similar arrangement of vessels in the legs of fowls, shewn by Dr Macartney, may be subservient, Mr Abernethy thinks, without being essential, to these modes of action.

Proceeding to the consideration of the hypothesis by which these phenomena have been attempted to be explained, Mr Abernethy rejects, first, the doctrine, that contraction is "the effect of some chemical change occurring in the part;" and, secondly, the opinion, that "the contraction of irritability is a property of the muscular fibres." His arguments in confutation of the latter opinion are not very satisfactory: they are three. First, he says, properties are generally considered as permanent qualities, continually operating, which is not the case with the contraction of irritability: secondly, the faculty is connected with different arrangements and qualities of matter, and not with one peculiar arrangement, as would be expected: and thirdly, as he believes the muscular fibres to be not continued from one end of the muscle to the other, so he thinks any breach of continuity would completely frustrate the contraction of the whole muscle, if irritability were a property belonging to the fibres;—an inference which we do not comprehend. The two preceding arguments are founded upon assumptions, as to the nature and connection of certain properties with certain conditions of matter, and are merely conjectural.

Having rejected the two hypotheses just mentioned, the lecturer proceeds "to inquire into Mr Hunter's opinion, that irritability is the effect of some subtile, mobile, invisible substance, superadded to the evident structure of muscles, or other forms of vegetable and animal matter, as magnetism is to iron, and as electricity is to various substances with which it may be connected." The principal, or rather the only argument adduced in support of this doctrine, is, that all matter possesses a *vis inertiæ*, or an indisposition to move unless impelled to motion, and a disposition to continue in motion unless retarded. Now the matter of animals and vegetables is common inert matter; *therefore*, "the necessity of supposing the superaddition of some subtile and mobile substance is apparent." p. 41.

In order to shew, then, that this superadded substance is the matter of electricity, which pervades all nature,

——agitat molem, et magno se corpore miscet,

Mr

Mr Abernethy reasons thus.

" Taking it for granted that the opinions generally entertained concerning the cause of electrical motions are true, analogy would induce us to suppose, that similar motions might be produced, by similar causes, in matter organized as it is found to be in the vegetable and animal systems.

" The phenomena of electricity and of life correspond. Electricity may be attached to, or inhere in, a wire; it may be suddenly dissipated, or have its powers annulled, or it may be removed by degrees or in portions, and the wire may remain less and less strongly electrified, in proportion as it is abstracted. So life inheres in vegetables and animals; it may sometimes be suddenly dissipated, or have its powers abolished, though in general it is lost by degrees, without any apparent change taking place in the structure; and in either case putrefaction begins when life terminates.

" The motions of electricity are characterized by their celerity and force; so are the motions of irritability. The motions of electricity are vibratory; so likewise are those of irritability. When by long continued exertion the power of muscles is fatigued, or when it is feeble, their vibratory or tremulous motions are manifest to common observation, but the same kind of motion may be perceived at all times by attention, as has been shewn by Dr Wollaston in the Croonian Lecture for the year 1810. It is then I think manifest, *that Mr Hunter's conjectures are the most probable of any that have been offered as to the cause of irritability.*" p. 41-3.

This is in reality the sum of the inferences, to which such a string of loose and imperfect analogies can possibly lead, viz. that Mr Hunter's conjectures possess a sort of probability, at least equal to the *suppositions* of other people! But surely if we reflect on the multitude of phenomena in the two organized kingdoms of nature, which bear no sort of resemblance to the operations of electricity, the inference in favour of the identity of the cause of these phenomena must be altogether inadmissible. But Mr Abernethy contends that the experiments of Sir Humphrey Davy have solved the great mystery of chemical attraction, by shewing its dependence upon the electric properties of all matter; by demonstrating that electricity controls the ordinary operations of nature, so as to make potash pass through a strong acid without forming any combination; and that it causes the whirlwind, and the water-spout, splits the oak, and levels the firm tower, and makes the earth tremble.

" When, therefore, we perceive in the universe at large," he adds, " a cause of rapid and powerful motions of masses of inert matter, may we not naturally conclude, that the inert molecules of vegetable and animal matter, may be made to move in a *similar manner* by a similar

similar cause?" and again, "If then it be electricity that produces all the chemical changes we so constantly observe in surrounding inanimate objects, analogy induces us to believe that *it is electricity which performs all the chemical operations in living bodies*; that the *universal chemist* resides in them, and exercises in some degree peculiar powers, because it possesses a peculiar apparatus." p. 50.

Now, though it is an admitted axiom in philosophy, that similar phenomena, occurring under similar circumstances, imply a similarity of cause; yet surely it is a great misapplication of terms, when either the phenomena or circumstances accompanying a thunder-storm, a water-spout, a volcanic explosion, or even an experiment with the Voltaic battery, are said to be *similar* to the functions of either animal or vegetable life. But either our faculties are very obtuse, or there is a considerable obscurity in the language of our lecturer; for, after the perusal of these passages, we were fully impressed with the opinion, which we stated in the outset, that Mr Abernethy maintained electricity to be life; nevertheless, immediately after the sentences just quoted, we find him asserting, that, "it is not meant to be affirmed that electricity is life." (p. 51.) What then is meant? Possibly the reader may discover it from the passage which follows.

"I only mean to prove," says Mr Abernethy, "that Mr Hunter's theory is verifiable, by shewing that a subtile substance of a quickly and powerfully mobile nature, seems to pervade every thing, and appears to be the life of the world; and therefore it is probable that a similar substance pervades organized bodies, and produces similar effects in them." (Ibid.)

Yet the lecture concludes by the statement of the author's opinion, that Sir Humphrey Davy has realized the hypothetical speculations of the Pythagoreans, by demonstrating the existence of the *anima mundi*, the Ψυχή, of these philosophers; and we had just been told that electricity was the "universal chemist" demonstrated by that distinguished philosopher. Perhaps, therefore, we are bound to infer, that it is not easy, even for men of genius, to think clearly, when they enter the labyrinths of speculation upon subjects that are actually without the bourne of human research.

The second lecture commences with a neat sketch of the structure and functions of the nerves, and of the opinions of physiologists respecting the uses of ganglia. Mr Abernethy traces the gradations in the complication of the nervous system, from the lower order of animals up to man, from the first appearance of a visceral nerve in the lowest creatures, that are possessed of organs for the preparation of nutriment, to the appearance of a nervous

vous cord, more or less beset with ganglia, and lastly to that of a kind of ganglion or brain at the extremity of this cord, which gradually becomes larger and more complex, as the series ascends to man: and he remarks on the extensive connection that is established among the vital organs, through the medium of the visceral nerve, which is thence denominated, in the human subject, the great sympathetic nerve. And thus from the brain, medulla spinalis, and visceral nerve all the parts of the body are amply supplied with nerves.

Mr Abernethy then proceeds to inquire how far Mr Hunter's theory of life may be adequate to explain the phenomena of the nervous functions. He first shows, by several facts and arguments, that the brain is the actual seat of all sensation, and the source of all volition; and thence, he maintains, "it necessarily follows that *motions* must be transmitted to and fro along the nervous cords, whenever they (sensation and volition) take place." The want of elasticity and tension, and the pulpy structure of the origins and terminations of the nerves, he observes, render the old suppositions, that these cords were capable of mechanical vibrations, inadmissible. It was afterwards conjectured, that the nerves were tubular, and that the communications were made between the sensorium and their extremities by the motions of a subtile fluid. This was the opinion of Haller, and Mr Abernethy considers it as a sensible opinion for the time. From this, indeed, Mr Hunter's supposition does not essentially differ; for he maintained the inherence of a subtile and mobile substance in the nerves, without conceiving them to be hollow tubes. This supposition of Mr Hunter's is made more specific by Mr Abernethy, who identifies it with electricity, or something so like electricity, that we cannot distinguish the difference. And this "theory of life," he thinks, "is a verifiable theory, and affords the most rational explanation of the phenomena of irritability, and of those nervous functions that have been considered." p. 87.

We cannot conceal our surprise, that this should be the serious opinion of any philosopher. Surely the various hypotheses alluded to are nothing but expressions of the fact, that between the sentient extremities of the nerves and the common sensorium there is a rapid and reciprocal communication, by means of which sensation and voluntary motion, and irritation and involuntary motion, are produced; but they add nothing to our knowledge of the essential nature of that communication; because we are acquainted with nothing exactly analogous to it in any form of unorganized matter. Whether we assert that the communication consists of intermediate vibrations, of a subtile fluid, of an electrical substance, or of an impulse propagated along the

nerves,

nerves, we therefore express only the *connection* or *sequence* of the internal sensation and the external impression;—a fact beyond which we shall, in all probability, never be able to proceed. The knowledge of this fact is all that is requisite to enable us to arrange the observations, which we make on the functions of vitality, *i. e.* in other words, to explain the phenomena of health and disease, and all the important sympathies, which Mr Hunter led the way in pointing out, and which are nothing but inexplicable connections between different organs, which the philosopher may record and arrange, and then he will have attained the summit of human knowledge. With this degree of knowledge we must be content, if not satisfied; for "fatiguing" as doubt and uncertainty may be to the human mind, the shadow can never supply the place of the substance, and it is surely but a shadow which we reach, when we overstep the limits just mentioned. These notions of philosophizing, however, our author professes not to understand; and he speaks in a tone of censure of those, to whom (from the adoption of similar views) no hypothesis is satisfactory, and who propose none themselves for the adoption of others. Such a proposal would, in fact, be incompatible with such opinions, and ought not to be imputed to any proud pretensions to "a philosophical turn of mind which exempts them from vulgar prejudices."

"I mention these things," Mr Abernethy observes, "because I am aware that there are some who say sympathy is a term without any direct meaning, and that all which Mr Hunter said on the subject of life explains nothing. What Mr Hunter meant, I believe I understand; what persons of different sentiments, whom I acknowledge to possess great information and ability, mean, when they talk in this manner, I am not so well able to discover. They seem to deny that life can be any thing which may be seen or felt. They seem to wish us to believe that they have that philosophical turn of mind, which exempts them from vulgar prejudices, and no theory appears to them satisfactory, neither do they propose any for our adoption." p. 91.

We shall not infer that Mr Abernethy intends to intimate, that a disposition to adopt these hypothesis has its foundation in vulgar prejudices, as that may not be deemed the legitimate converse of his proposition. But we do not perceive the applicability of the following aphorism to the subject in question, although its correctness may be admitted; that "in the uncertainty of opinions, wisdom would counsel us to adopt those which have a tendency to produce beneficial actions."—p. 93. For it is not very obvious, that the conduct of mankind can be much influenced by the speculations of physiologists about a vital principle,

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or that the existence or non-existence of such a superadded substance, as distinct from the corporeal and intellectual part, can be the foundation of any practical consequences.

Notwithstanding the high names, therefore, by which this mode of theorizing is sanctioned, and notwithstanding our unfeigned respect for the talents of the present advocate in its favour, we continue to believe that it is at variance with the right rules of philosophy, that it contributes to add nothing to the actual stock of knowledge, and that the collection of analogies and distant resemblances in objects essentially different in a much greater number of circumstances, is an exercise rather calculated to form the poet, than the interpreter of nature.

II.

An Account of a successful Method of treating Diseases of the Spine; with Observations and Cases in Illustration. By THOMAS BAYNTON, of Bristol. 8vo. pp. 125. Bristol, 1813.

MOST physicians and surgeons must have seen enough of the application of setons and issues, to convince them that such remedies will not cure diseases of the spine; that very often they do no good, and sometimes positively do harm, by the great pain and irritation which they excite, so as to make it necessary to desist from the use of them altogether. It is impossible not to distrust the testimonials, strong as these of course are, in favour of the practice recommended by Pott, Earle, and other distinguished men. Mr Baynton has, therefore, rendered an important service to his profession by the publication of this memoir, in which he has shewn that a mild method of treating diseases of the spine is to be relied on, and preferred to the harsh and severe plan which is generally employed. By quotations from the writings of Pott and Earle, he shows that drains were not sufficiently efficacious to cure caries of the spine without the removal of the superincumbent pressure, either by rest for a a year or two in bed, or by machinery; and by the histories of several cases under his own observation, he has ascertained that the observance of strict undeviating rest alone, in the horizontal position, is more efficacious. In some instances, the muriate of lime and cinchona were occasionally exhibited, and in the latter stages, and in the more chronic form of scrofula, Mr Bayn-

ton

ton is convinced by experience, that the muriate of lime will prove eminently beneficial, especially among the sickly poor. We select his description of the crib, and the means whereby excoriation and other inconveniences occasioned by long confinement can be avoided, and quote one case,—referring our readers who are most interested on this subject to the little tract, which contains other facts, on the utility of which the author rests all his claims for approbation.

"A crib, or narrow bedstead, must be procured, six feet in length, or rather of a sufficient length to accommodate the patient; two feet one inch in height from the floor of the apartment to the floor of the crib, whereon the mattress is placed; two feet five inches wide, with posts three feet seven inches high, (including castors) to be turned (by a turner) as a common crib.

"It must be provided with a rail floor, instead of sacking; and with side boards to raise up and down; which, when half raised, will resemble the raised flap of a table, and must be supported with sliders, that can be drawn in or out when required; and which, when wholly raised, will furnish sides to the crib, for security, or warmth at night. The castors should be of brass, and of the strongest description.

"This crib is to be fitted with a mattress, from three to four inches thick, that has been French, or double stuffed, with the best horse hair; made two inches shorter, and two inches narrower than the crib, in the clear of the sides, head, and foot board; for the purpose of affording room for raising the sides, and turning in the bedclothes. Its width will be sufficient for the accommodation of the patient, though its dimensions will admit it to be drawn through the door-ways of the patient's day and night apartments. Its height, when the mattress is placed upon it, will be just sufficient to raise the patient to the level of a common table."

"Master William Castle, son of Thomas Castle, Esq. of Portland-Square, Bristol, when about a year old, was observed to have lost his health, and it was discovered that he could not move his legs as well as he had been accustomed to do. On examining the spine I discovered that four of the lower dorsal vertebræ were in a diseased and protruded state, and that great pain was occasioned by moderate compression, either of the bones or contiguous soft parts. Absolute rest on a hair mattress was strictly enjoined. The muriate of lime, in appropriate doses, directed, and the constant care of a confidential servant ensured. The advantages resulting from those means were soon apparent; they continued progressively to increase till the cure was accomplished. At the end of the tenth month he was allowed to roll and play on a carpet; in a short time after he got on his feet without assistance, and has continued in perfect health from that time to the present. The curve is entirely removed."

Some cases of failure are candidly related, which prove that diseases

diseases of the spine and of the hip-joint will sometimes get well, and sometimes not, whatever means may be employed for their cure. Mr Baynton has practical reasons for doubting whether the cure of the hip-joint disease can safely be entrusted to the effects of rest alone. He seems to think there is a difference between affections of the hip and of the spine; that the order of morbid action is reversed; in one (the hip-joint disease) the soft parts are the first affected, in the other, (caries of the spine) the bodies of the vertebræ are primarily diseased. We are disposed to believe that these diseases are the same; that in the scrofulous caries of the hip, and in the scrofulous caries of the spine, the ulceration of the cartilages is the primary affection, and the other neighbouring parts become affected only in a secondary manner. There can be no doubt that the spine and the hip, and other joints, are subject to many morbid affections, some of which have been well described by Mr Brodie in his excellent paper in the Medico-Chirurgical Transactions, Vol. 4th, noticed in our last number, but the symptoms described by Mr Pott and Mr Ford, are produced by the disease beginning in the cartilages, and have been supposed to be cured by the application of issues and setons, recommended by those writers. Yet what a mass of irreconcilable contradictions does the result of all their practice present. After describing the formidable nature of the disease, and the means employed to restore the use of the limb, or preserve life, Mr Ford relates a case of disease of the hip-joint, where the symptoms disappeared without any external application, or the use of any internal medicines, (vide Case XII. page 163, first edition.) The first case mentioned by Mr Pott, died of small-pox in a few weeks after issues were made upon the back; the second recovered in six or seven weeks, and would not submit any longer to endure the issues. This patient attributed his cure, as he well might, to confrey-root tea, and isinglass. In one place we are told, that "no assistance is to be expected when the bodies of the vertebræ are completely carious," and in another, "that issues are capable of effecting a cure, even after a caries has taken place, and that to a considerable degree," "*which is true to demonstration.*"

If one will cautiously examine the "*Select cases*" published by Dr Jebb, to confirm Mr Pott's theory and practice in paralysis of the lower extremities, he will find, that, out of fifteen cases, six died, five were cured, in one caustics produced no effect, and the other three were not cases of diseased spine.

That some patients have recovered, whilst caustics were applied to their backs, is certain, just as happened to many who had the same disease before issues were introduced into practice,

and

and as has happened to many patients affected with carious spine and palsy of the lower limbs, who were not subjected to this treatment. General tonics and sea-bathing have sometimes gained the credit of effecting the cure without issues; and, in that case, it is said, nature, unassisted, restores the use of the limbs; yet, we are disposed to think, the best assistance is rendered to the efforts of nature in such instances, by good air, generous diet and careful nursing. If the method recommended by Mr Baynton be admitted to be the best for managing diseases of the spine, there appears one great objection to it, viz. the difficulty of keeping a child so long confined to one posture. Should the child be in good health, it will continually be getting upon its legs; should it be sickly, it will always be wanting some change of position, besides being deprived of air and exercise, which, in many cases, seem to be essentially necessary to preserve life. This subject is very important, and must soon undergo a complete discussion. We refer our readers to two valuable communications to our Journal, one in the 8th volume by Mr Burroughs, and the other in the 9th volume, by Dr Armstrong. The observations made by those intelligent practitioners so entirely coincide with our own experience, that our review of this subject is much abridged, by a reference to what has already been published in our pages.

III.

Practical Remarks on Diseases resembling Syphilis, with Cases. By JOHN WHITSED, Surgeon, Peterborough, 8vo. pp. 50. 1813.

THE cases related in this little tract, confirm the accuracy of Mr Abernethy's observations respecting diseases which resemble syphilis in their symptoms, but which require no mercury for their cure. In every case of pseudo-syphilitic swelling or sore, Mr Whitsed has found the general health more or less disordered, and mercury rather aggravates than relieves the complaint, by adding another cause of irritation to the constitution. The whole subject is still involved in obscurity. An extensive collection of facts will in time lead to some more satisfactory diagnosis than what at present exists, and therefore every contribution to so desirable an object merits attention and praise.

IV.

IV.

Traité des Hæmorrhoïdes. Par JOSEPH BRICE DE LARROQUE Docteur en Médecine de la Faculté de Paris. 1813. pp. 270. 8vo.

PHYSICIANS on the Continent have always considered the piles as an important disease. Their writings contain frequent allusions to the influence of hæmorrhoidal bleeding on the constitution; and most of their writers inculcate, that the discharge is salutary, to repel it dangerous, and to promote it the best method of relieving various diseases of the viscera and head. Their general views of the causes, and of the effects of hæmorrhois, have, however, always appeared to us unsatisfactory; and we have been pleased to meet with a better explanation of the symptoms, and a more simple method of cure, in this work, than in any publication upon this subject.

Morbid anatomy teaches us, says Dr De Larroque, that hæmorrhoidal tumours are of two kinds; sometimes they consist merely of cellular or spongy texture, as Le Dran observed; at other times they are encysted tumours, as M. Recamier has pointed out. The blood is discharged by exhalation, not from the rupture of any obstructed or enlarged arteries or veins; the inner membrane of the rectum, and the inside of the tumours, situated on the verge of the anus, have the same appearance in cases of hæmorrhoidal flux which terminate fatally, as the inside of the uterus in menorrhagia; and the author concludes, from his examination of several dead bodies, and from a comparison of all the facts connected with this disease, that the hæmorrhoidal flux arises from exhalation (or *percolation*) like epistaxis, hæmoptysis, hematemesis, hematuria, and menorrhagia. The only difference between these several hæmorrhages is in their situation.

His method of treatment does not differ much from what is recommended by our best practitioners. Besides general and topical bleeding, and a low diet, he advises the use of glysters and lotions of cold water, or vinegar and water, which have removed the pain, in cases even where no hæmorrhage occurred, after every thing else failed. Opiates he condemns; he praises the tartrite of potash, and the sulphat of magnesia; and for the removal of the tumours he prefers the knife to the ligature.

V.

An Essay on the Signs of Murder in new born Children, Translated from the French of Dr P. A. O. Mahon, Professor of Forensic Medicine in the Medical School at Paris, &c. &c. By CHRISTOPHER JOHNSON, Surgeon, Lancaster. Lancaster, 1813. pp. 114.

WE are glad to see the attention of the profession more directed to the study of Forensic Medicine; for medical practitioners frequently fail in the performance of their duty, when examined in criminal cases, not from professional ignorance, or want of zeal, but solely from the novelty of their situation, and never having considered the subject in that particular point of view which is necessary to elucidate the doubts of a court of law.

Child-murder is one of the most frequent subjects of trial, and it is particularly interesting, on account of the great difficulties in which it is involved, and on account of the erroneous opinions in regard to it, propagated by Dr William Hunter, which are now first controverted in the English language, by Mr Johnson, in his preface.

The publication itself is a translation of a chapter of Mahon's posthumous work on legal medicine. We shall briefly notice its contents. P. Mahon first treats of the circumstances which commmonly constitute the defence of a woman accused of child-murder; the uncertainty of the signs of pregnancy; the rapid progress of labour; the impossibility of affording the infant the necessary assistance. He then discusses the following questions:

"1*st*, Was the child capable of living after birth? 2*d*, Was it alive or dead at the commencement of labour? 3*d*, Was it dead or living when born? Did it live any length of time after birth? 4*th*, What were the causes of its death? Did they occur before its birth or afterwards? 5*th*, Was the woman really delivered at the time supposed? 6*th*, What time has elapsed since the delivery took place?"

The second chapter treats of the signs of child-murder, derived from an examination of the umbilical cord; the third of the Docimasia pulmonum, and of the circumstances which may render it ambiguous; and the last of the manner of dissecting the fœtus, so as to render the examination as satisfactory as possible. To the whole, the translator has subjoined a few notes, and we trust that he will be successful in calling the attention of the profession to a very difficult subject, which still requires much investigation.

VI.

VI.

Official Papers relating to Operations performed by Order of the Directors of the Royal Hospital for Seamen at Greenwich, on several of the Pensioners belonging thereto, for the purpose of ascertaining the general efficacy of the new modes of Treatment practised by Mr ADAMS, *for the Cure of the various Species of Cataract, and the Egyptian Ophthalmia.* London, 1814. pp. 21.

We cannot praise Mr, now Sir William, Adams. We can conceive no reason not unworthy of a member of a liberal profession for keeping secret new modes of treatment, practices, or operations, after their efficacy is ascertained, and when, by disclosing them, he could confer a benefit on the human race. If we were not well acquainted with the history of *nostrums*, we might be inclined to think the evidence here laid before us conclusive; but we shall fear some mistake or deception until the practices shall be confirmed by their success in other hands than the knight's, for it "is the *operation* and not the *operator*" we look to, and until by the disclosure of the secrets we shall be enabled to judge of their merits scientifically.

VII.

Ueber das Wesen und die Bedeutung der Exantheme. Eine philosophisch-medicinische Abhandlung. Von Dr D. G. KIESER. 4to. Jena, 1812. pp. 44.

This pamphlet fell into our hands very opportunely, as it illustrates most strikingly some of our remarks upon Mr Abernethy's defence of Mr Hunter's Theory of Life. The book professedly treats of *exanthema*, its essence and meaning, but opens with a theory of life of that transcendental nature, which completely eludes the powers of our vulgar comprehension.

"What is, and wherein consists all life, the lowest as well as the highest, the celestial life of the stars as well as the terrestrial of earthly beings, the psychical life of the soul, as well as the physical of the body, except in an oscillation between two antagonist points? And

And what is the vital principle in its highest acceptation but the organic tension which maintains this oscillation? What further are all the different states and epochs of life, but different moments of this oscillation, by which one of the antagonist points preponderates more or less, and thus revives and supports the oscillation."

"Thus is eternal war the source of life, and eternal peace would be the death of every thing that lives. It is only by, and in, this war that life exists, only in the most violent strife of antagonist powers that the flowers of beauty expand, and the buds of life burst into vigorous foliage." p. 1.

And such bombastical nonsense is dignified with the titles of philosophy and eloquence, and is pronounced by a German professor, as a preliminary oration to a course of pathology. The theory of disease is equally sublime.

"As all life merely consists in the oscillation of organic tension, so does each of its states, and so does disease. Every disease in its general form is a minor oscillation, produced by organic tension in the greater oscillation of the life affected by the disease, just as the satellites repeat similar oscillations within the greater oscillation (elliptic orbit,) of the planets, and as the leaves of a plant represent the same organic tension by which the whole plant is developed. Hence every completed disease, in its general form, repeats and represents the epochs of life in general."

But the end of this oscillation or vital process, is to bring the antagonist points into coincidence, and from this union to regenerate a stronger oscillation. In short, for it would be absurd to follow our author's fancies and extravagancies, every thing that lives necessarily dies many times in the course of its life to revive more perfect; each of these internal changes is attended by an external change of form or metamorphosis. In the vegetable kingdom, the budding, expansion, and fall of the leaf, are the minor oscillations, tending to the developement and perfection of the plant. In the lower animals, the progression of their nature is strongly marked by change of external form; but in man these regenerations are indicated by the production of exanthematous diseases; so that, in fact, our author's "transcendental theory" of cutaneous diseases resolves itself into this,—that a new born infant is a *maggot*, which changes its skin in completing those oscillations, whose periods are marked by the production of small-pox and measles; becomes a *pupa* when it passes through scarlatina; and has no pretensions to the denomination of a perfect man, until the final divergence of the antagonist points from that dangerous coincidence which gives rise to his last metamorphosis by the spotted fever!!!

PART

PART III.

MEDICAL INTELLIGENCE.

Report of the Diseases treated at the PUBLIC DISPENSARY, *(near Carey-Street,) London, from February 28, to May 31, 1814.*

Acute Diseases.

	No. of Cases.		No. of Cases.
Tertiana	1	Peripneumonia notha	3
Quotidiana	1	Pertussis	16
Febris	16	Cholera	1
——— infantum rem.	13	Enteritis	1
Hydrocephalus	3	Peritonitis	3
Cynanche tonsillaris	9	Podagra	1
——— parotidæa	2	Rheumatismus acutus	6
Epistaxis	1	Arthritis rheumat.	6
Catarrhus	111	Rubeola	3
Pleuritis	3	Erysipelas	1
Peripneumonia	13		

Chronic Diseases.

Cephalæa	37	Amenorrhœa	2
Vertigo	13	Chlorosis	2
Catarrhus chronicus	61	Nephralgia	7
Hæmoptysis	18	Dysuria	3
Phthisis	15	Hæmaturia	2
Palpitatio	8	Rheumatismus chronicus	34
Hæmatemesis	3	Pleurodyne	4
Gastrodynia	16	Lumbago	5
Dyspepsia	43	Hysteria	5
Icterus	6	Epilepsia	3
Calculus biliosus	1	Asthenia	19
Diarrhœa	10	Hemiplegia	2
Dysenteria	13	Paraplegia	1
Constipatio	7	Livor manuum	1
Enterodynia	14	Chorea	1
Tympanites	1	Anasarca	5
Physconia	1	Hydrothorax	1
Ascarides	2	Struma	4
Tænia	2	Scorbutus	1
Hæmorrhois	3	Strophulus	1
Marasmus	11	Lichen	3
Hysteralgia	4	Lepra	3
Leucorrhœa	8	Psoriasis	4

Chronic Diseases continued.

	No. of Cases.		No. of Cases.
Urticaria	3	Ecthyma	1
Purpura	3	Scabies	8
Porrigo	16	Rupia	1
Impetigo	3	Herpes	1

The severe weather continued without interruption till the 20th of March, when the frost suddenly disappeared, on the occurrence of a west wind. From that time, until the latter part of April, the temperature of the atmosphere was extremely mild, and even occasionally hot; but the wind again returned to the east, in which quarter it has continued to the present time, bringing with it incessant cold, a cloudy atmosphere, and some chilling rains; so that vegetation, which was advancing luxuriantly, has been much retarded and blighted.

This will account for the continued prevalence of catarrhal and pulmonary diseases, which have been unusually numerous and severe, as well as for the rheumatic complaints, which have also been violent and tedious. The hooping-cough, which has been prevalent, has been also apparently aggravated by the same cause. The disorders of the digestive organs, including not only dyspepsia, gastrodynia, &c., but also the majority of the cases of headache, and vertigo, which are sympathetic of derangement of the stomach and bowels, are always so numerous in this metropolis, that the large proportion, which appears in the preceding list, may be independent of the state of the season. They are more commonly, indeed, aggravated by the opposite state of temperature, the autumnal heat. But it is probable, that the scarcity of provisions during the long winter, and the substitution of unwholesome stimulants for nutritious food, may have contributed to increase the catalogue of these diseases among the poor. The single case of cholera occurred during the few hot days in the beginning of April; but even this disorder occasionally attacks individuals of bilious temperament, under any extraordinary disturbance of the digestive organs by over-excitement, independently of external heat. In a considerable majority of the cases of gastrodynia, the pain is particularly severe after taking food, and in a few, it is chiefly distressing when the stomach is empty. In the latter instances, cordials, bitters, and aromatics relieve the pain, by giving tone to the stomach, which chiefly suffers when there is a deficiency of stimulus: but in the former, anodynes are absolutely necessary, especially such as do not constipate the bowels, as the conium and hyoscyamus, and the preparations of the humulus, which combines an aromatic with its anodyne power. In these cases, there is often a strong tendency to the generation of acid in the stomach, and also to constipation; whence alkaline and laxative medicines constitute a valuable combination with these anodynes,

anodynes, in this form of gastrodynia, as well as in the pyrosis, which not unfrequently accompanies it.

The case of scorbutus occurred in a girl, ten years old, and was marked by an extremely spongy condition of the gums, which were so tumid in some parts, as nearly to conceal the teeth, and bled freely when pressed or rubbed, and in the act of eating any thing that was not soft. There were no ecchymoses or spontaneous extravasations, but a purplish redness was produced by much pressure of one limb against another, especially inside the knees. She was weak and languid, and complained of a severe aching pain in the lower extremities, which induced her to lie much on the bed. For three years back, this girl has not tasted any fresh vegetables, even potatoes, and altogether refuses all vegetable food in its fresh state: as her parents were unable to supply her with animal food daily, though they indulge her with it as often as they can, from her extreme aversion to vegetables, she lives principally upon what they call broth, made of butter, salt, and water, and upon bread and butter. She is particularly fond of salted meats. During a part of the period since her rejection of vegetables, she resided in Gloucestershire, where she drunk cyder daily; and during this time she did not suffer any scorbutic symptoms; but on coming to London these symptoms recurred. The bowels are regular; she has no fever; the appetite is good; and she has an appearance of tolerable health; but the pain and debility of the limbs prevent her from taking active exercise. Purging has appeared decidedly to augment the symptoms, especially the pains of the limbs, and the disposition to hæmorrhage from the mouth; whilst, on the other hand, she seems to be materially relieved in both these respects from the use of the decoction of cinchona with the sulphuric acid. In one of the cases of purpura which is now under treatment at the dispensary, the opposite result has been observed from the administration of these medicines. The patient is a middle-aged woman, of a thin habit, and the eruption consists of a numerous crop of petechiæ, of various sizes, nearly covering the legs and feet. These have appeared copiously in succession, for three or four weeks, and have been accompanied with considerable depression of the strength and spirits, and a slight tendency to œdema. The gums have not been affected in the slightest degree, nor has any hæmorrhagic disposition shown itself. All the symptoms have been invariably relieved by purging with calomel and jalap; she distinctly stated the regular relief to her feelings after each repetition of the dose, and pointed out the obvious diminution of the eruption, and the cessation of its tendency to appear again, at the same time; her depression of strength and spirits was particularly diminished by this operation. But when these medicines

medicines were omitted, and cinchona, with sulphuric acid, was given, for the purpose of aiding the convalescence, the crops of petechiæ returned, and with them the debility and languor. There is a species of purpura, however, which occurs in old people, and principally, if not exclusively in old women, affecting the fore-arm, with very irregular, and sometimes large dark purple blotches, in which purging, and indeed every other remedy that I have used, appears to have no salutary influence. The eruption continues to appear, in a constant series, or at short intervals, for a long period, even for several years, without any material alteration in the general health. In one case, from the extraordinary hardness and vigour of the pulse, a moderate bleeding was employed; but though the pulse became soft, the eruption was not diminished, and the strength of the constitution seemed afterwards to decline. T. B.

Bloomsbury Square, May 31, 1814.

Since the publication of our last number, we have received the following books, for which we take this opportunity of returning thanks:

A practical Account of the fever commonly called the Bilious Remittent, as it appeared in the ships and hospitals of the Mediterranean fleet; with Cases and Dissections. To which are added, Facts and Observations, illustrative of the causes, symptoms, and treatment of fever in the Mediterranean; comprehending the history of fever in the fleet, during the years 1810, 1811, 1812, 1813; and of the Gibraltar and Carthagena fevers. By William Burnett, M. D. physician of the fleet, &c. London 1814. Price 10s. 6d.

An Essay on the Venereal Diseases which have been confounded with Syphilis, and the symptoms which exclusively arise from that poison. Illustrated by drawings of the cutaneous eruptions of true Syphilis, and the resembling Diseases. By Richard Carmichael, M. R. I. A. president R. C. S. in Ireland, &c. Dublin 1814. Price 10s. 6d.

Observations on those Diseases of Females which are attended by Discharges. Illustrated by Copperplates of the diseases, &c. By Charles Mansfield Clarke, M. R. C. S. &c. London, 1814.

A practical Essay on the diseases of the Vessels and Glands of the Absorbent System: being the substance of observations which obtained the prize for 1812, offered by the Royal College of Surgeons in London: to which are added surgical cases, with practical remarks. By William Goodlad, surgeon, Bury, Lancashire, member of the College, &c. London 1814. Price 7s. 6d.

*** We have received communications from Dr Calvert, Dr Laing, Mr Salter, Mr Hartte, Mr Sankey, and Mr Bakewell.

*** Communications may be addressed to the Editors, to the care of Messrs Constable & Co. Edinburgh; Messrs Longman, Hurst, Rees, Orme, & Brown, London; and Gilbert & Hodges, Dublin.

No. XL. will be Published on the 1*st of October* 1814.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. OCTOBER 1814.

PART I.

ORIGINAL COMMUNICATIONS.

I.

Observations on Phlegmatia Dolens. By Mr William Sankey, Surgeon.

THAT complaint called by Dr Hull Phlegmatia Dolens, has been so often, and so well described, that it is not likely any thing I can say will make it better understood; nor should I have offered myself to your notice, but for a paper on the subject by Dr Wyer, in No. 134 of the Medical and Physical Journal, in which the history, symptoms, and mode of treatment nearly agree with my observations, except in the use of blisters, which he did not think of till his patient was nearly well. The effect was such, as to make him regret he had not used them before. As it is a remedy I have made use of for 14 or 15 years with much advantage, I think I shall be excused for troubling you with the result of my observations. I confess myself totally ignorant of the cause of the complaint; for, like Dr Wyer, I have not found it follow any particular kind of labour, confined to any season of the year, or to any particular habit of body, though I think it more likely to attack the weak than the robust. I have never seen it follow the first labour, or attack the same patient twice. I have met with two cases in young women, not after parturition; both were severe,

severe, and well marked; both had obstructed menses, and one had a suppression of urine. I have also seen one well-marked case in a man 60 years old. It is not a very common complaint; in my practice it has happened once in about 200 times, excluding those not following parturition. In Dr Wyer's, and in most of the cases described by Dr Hull, Dr Ferriar, and I think Dr White, it was confined to one leg only. Most of my patients have had both legs affected, not at the same time, but after going through the progress he describes in one, the other becomes affected; and (unless prevented by the application of blisters) goes through the same stages, and takes the same time as the first. When I began practice, I tried every kind of topical application, and, like Dr Wyer, with the same disappointment. I have always found gentle laxatives useful and necessary, even when there is diarrhœa. To ease the pain, I have given small doses of opium with antimony; every other internal treatment has been directed to palliate symptoms. As there is always considerable debility, accompanying or following—generally want of appetite, attended with a quick and feeble pulse,—I have given some kind of tonic as soon as the stomach and state of fever would admit; but all these seem only auxiliaries, and not very important, except opening the bowels, and moderate doses of opium. What I consider as a specific, is a blister applied to the calf of the leg immediately upon discovering the complaint. Living in the country, and the complaint generally coming on after I have taken leave of my patient, I am not often called in till it has made some progress. Under these circumstances, it is often some time before the complaint can be subdued, though every blister is seen to relieve the pain, and, when thus completely formed, it often requires several blisters. The first I apply to the calf of the leg, as the pain is generally most severe in that part, and there is less fear of its not healing than if applied lower. If required, I repeat them every two or three days, not at the same place, but higher or lower, according to the seat of the pain. With every attention, this complaint often leaves considerable weakness in the leg most affected, requiring bandage or laced stocking. Though it is often a tedious and painful complaint, I have never seen it fatal.

Wingham, Kent, April 3, 1814.

II.

II.

Report on the Use of Charcoal Powder as a substitute for Cinchona.

By R. CALVERT, Physician to the Forces.

To the Editor of the Edinburgh Medical and Surgical Journal.

SIR,—When I sent you a former paper on the subject of Charcoal Powder, which I see published in your number for January, I intended to have prosecuted the inquiry, and to have sent you the result of my experience from time to time, in order to confirm or disprove, as such experience might dictate to me, what I had therein stated. But being called to Malta immediately afterwards, in consequence of the plague that infested that island, I have had but little opportunity of putting the virtues of charcoal to the test in intermittent fevers. The investigation, however, has in nowise been retarded in Sicily, from my leaving it. Dr Calcagno, to whom I am indebted for the discovery, has published the result of his further experience on the subject, in a letter "Sull' uso interno del Carbon di Legno," &c. wherein he relates his having cured a double tertian in the person of Giuseppe Bellavia, a patient in his hospital, by administering the powder of charcoal in the dose of a drachm every hour during the intermissions, but without the addition of an acid, which he formerly thought necessary. The fever did not return after two ounces of the powder had been administered.

From the 23d September to the 13th October, he had an opportunity of employing it in five other cases, in four of which the remedy succeeded by the time two ounces and a half had been given; but in the fifth, the fever not being removed after the exhibition of three ounces, he judged it prudent to have recourse to bark, which prevented the following paroxysm, although but half an ounce of the latter had been prescribed. The period of this paroxysm being over, he again ordered the charcoal, and the patient continued well.

Maccadino, his pupil, on returning to Calatafimi, his native place, in the month of August, succeeded in curing eight cases of intermittent fever with the same remedy; and Signior Francesco Buscarelli, surgeon, following his example, found it useful in four other cases of the like nature.

"In the district of Aliminusa, (says Dr Calcagno) I know

that

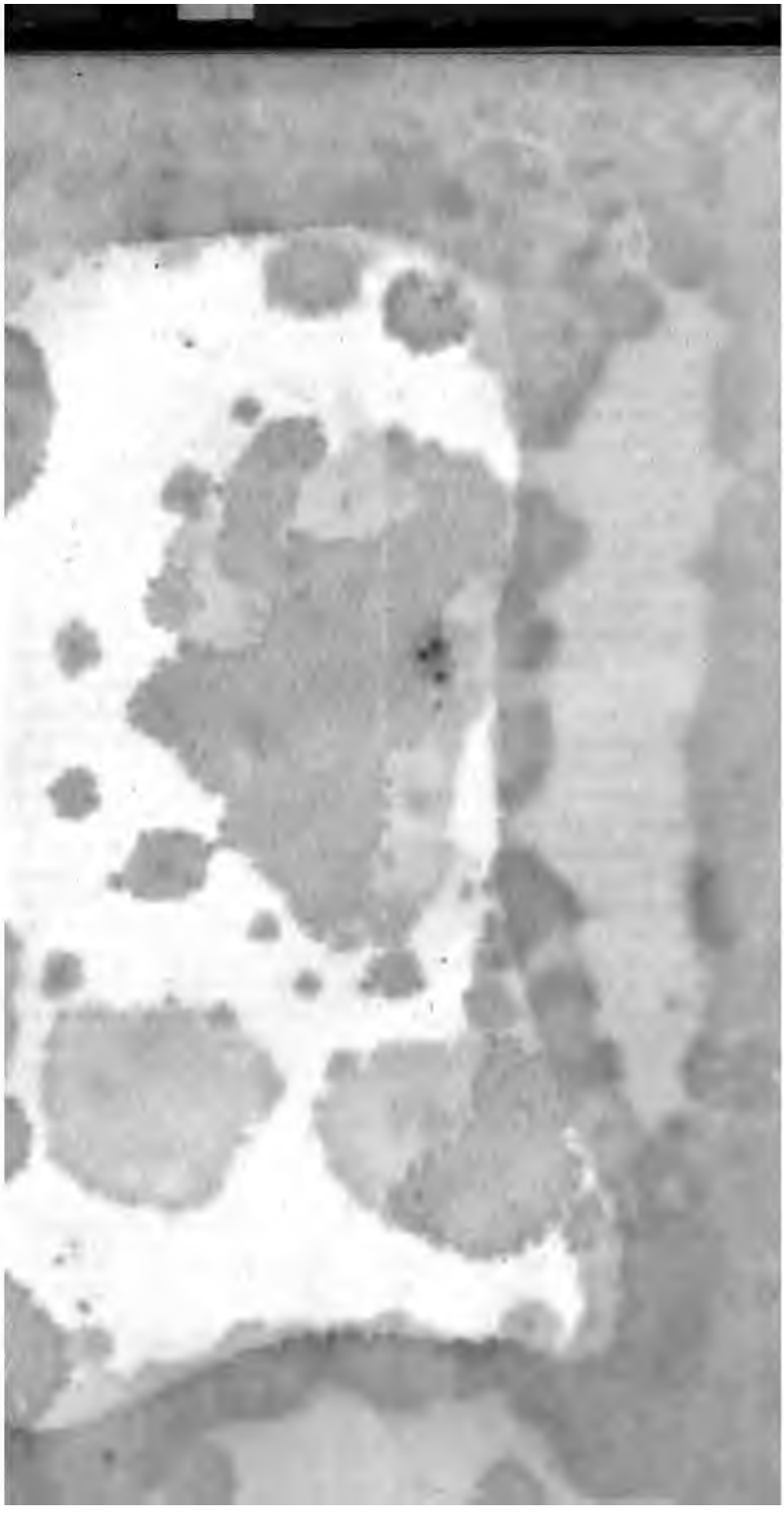
that the Journal * had scarcely arrived, when the practice became general, the inhabitants adopting it (the charcoal) as well without the prescription of the physician as with it; and these write to me in a letter, bearing date the 23d December, 'That the patients labouring under intermittent fevers that have been cured at this place by means of the powder of charcoal amount to a hundred and five; that each has not taken more than two ounces of the charcoal; and that the charcoal made use of was that prepared from the *stinco*.'" Dr Nicosia also employed it in 17 other cases in the district of Nissoria, according to the testimony of Dr Burza of Palermo. †

But, however assiduous the Sicilians may have been in taking advantage of a remedy they very much required, they have not been without rivals in many of British practitioners, under the auspices and control of Dr Borland, inspector of army hospitals. Mr Mackesy, surgeon of the 62d regiment, has permitted to be published in Italian seven interesting cases, in which the charcoal was successfully employed by him; but, as these cases will probably be published in England, I shall not enter into particulars respecting them. Mr Tully, surgeon 35th regiment, in a report made to the inspector of hospitals, dated Zante, November 12, 1813, states, that he has found the remedy particularly successful in remittent and intermittent fevers of that island; and that its administration, in more than thirty cases, principally intermittents, has been attended with the most happy effects. "In obstinate cases of diarrhœa, and in the last stages of dysentery," he adds, "I have seen that the charcoal, in doses of from 15 to 20 grains given three or four times a-day, is a most excellent remedy." This accords with my own experience, as related in the Journal for January. I shall relate a complicated case of intermittent fever and dysentery that occurred to me. It is to be remarked, however, that I did not prescribe for the man till the 29th July.

CASE.—ADAM HOPFFE, 20th Light Dragoons, æt. 37, orderly servant in the hospital; began to feel weak and languid in the middle of July, but did not complain till a week had elapsed, when, after attending a patient who died of dysentery, he was seized with headach, irregular chills, and other febrile symptoms, accompanied with a frequent purging of blood. He then took,

* The newspaper in which the discovery of the effects of charcoal was first announced.

† See Littera sull' uso interno del Carbon di Legno nelle febbre intermittenti, p. 11.



took, by order of the assistant, a dose of salts, which operated well. The purging continuing the next day, he took a grain of opium in the morning, and another in the evening. This gave him but temporary relief. Two days afterwards he took 15 grains of rhubarb, but the dysenteric symptoms continued. On the 29th he took three ounces of olive oil, which seemed to remove the symptoms of dysentery; but nausea and vomiting supervened, and, on the evening of the 30th, he was attacked with a regular paroxysm of ague.

On the morning of the 31st he complained of violent headach and weakness. The nausea and vomiting returned, and his appearance indicated considerable bodily disorder.

Sumat pulveris carbonis ℈i. statim, et vespere.

August 1.—His appearance is much changed for the better. He has had no fever nor vomiting since yesterday morning; appetite returning; bowels perfectly regular; a great many eruptions have appeared on his lips.

Continuatur pulvis carbonis.

I am perfectly aware that the administration of charcoal powder internally is by no means new. I myself have seen it prescribed in the Royal Infirmary at Edinburgh, and it has been recommended by a number of foreign authors;* yet, as it has never been recommended with the intention herein set forth, our knowledge of these, its qualities, may be fairly considered as a discovery. But had its virtues been perfectly known before, its present neglect, contrasted with the foregoing evidence, is sufficient to justify me in urging it as strongly as possible upon the attention of the medical world.

Lazaretto, Leghorn, April 30, 1814.

III.

Case of Singular Affection of the Mamma, cured by the use of Iron and of the Kali Purum. By Thomas Salter, Esq. Surgeon, Poole, Member of the Royal College of Surgeons in London.

M. W. aged 22 years, of a phlegmatic temperament, and having what Darwin calls a bombycinous colour of the skin, applied

* *Vide Moench*, Thomann, Horn, Brachet, Brugnatelli, &c. &c.

applied to me for the relief of a formidable disease which she had in both breasts, and concerning the origin and progress of which she gave me the following account. About two years and a half ago, when in good health, she was attacked in the left breast with inflammation. This was soon followed by suppuration. The matter her medical attendant discharged by puncture. The pus was good; and the symptoms attending its formation, were, as far as she could judge, precisely similar to those attending a common abscess. The case went on well until the granulations rose to the common level of the breast; when, instead of cicatrizing, tubercles formed on the surface of the sore. The surrounding healthy skin now ulcerated, and tubercles formed on it also. In this manner the disease soon became extended over nearly the whole of the breast. About the same time, lancinating pains began to be felt, and repeated and profuse discharges of blood took place. No healthy pus was formed; but there was a thin ichorous discharge of a very offensive smell. The tubercles were about the size of a small nipple, their apices of a light colour, and moderately hard. When touched they bled. The basis of many approximated so as nearly to unite. The bases and interstices were covered with coagulated blood, and matter of dark colour, approaching to black, which adhered tenaciously, and when removed, a bright red surface was exposed, from which blood would also flow. The skin, forming the circumference of the sore, had a bright red blush or areola. The lymphatic glands in the axilla occasionally became painful and tumefied, and once suppurated, but the part healed soon with little difficulty. Except the slight redness above mentioned, the surrounding integuments, previous to ulcerating, shewed no visible signs of disease. The breast has in every part a natural feel; nor has there been at any time the least lump or hardness in it; though there appears a general enlargement of the part, which is most evident superiorly, immediately below the clavicle. A few months subsequent to the commencement of disease in this breast, the other became affected in a similar manner, except that matter formed at the same time in two distinct situations; and that the tubercles did not spread over such an extent of surface. The patient at this time was greatly reduced by the intensity of the pain and the excessive discharge. The catamenia had disappeared for some time. The pulse was frequent and feeble, and the appetite bad; but she had regular and proper alvine evacuations. I did not flatter myself that I should be so fortunate as to afford her much relief, since she had already spent two years at two respectable county hospitals, where all the skill and attention they are so well calculated to afford had proved of no avail.

On

On the contrary, the disease did not cease to extend itself; and the surgeons, under whose care she had fallen, advised, as the only probable means of saving her life, that both mammæ should be extirpated. To this, however, she would not consent; but returned home with the gloomy prospects which the idea of having an incurable disease is naturally wont to excite. At this time I had not read Mr Carmichael's work on cancer; but I knew of his having employed, in what were considered as cancerous cases, ferruginous medicines, with very beneficial effects. I therefore directed finely levigated carbonate of iron to be sprinkled upon the diseased part night and morning, covering it with a plaster of the unguentum cetacei. She was also ordered to take the pil. ferri cum myrrha with a small proportion of aloes. This treatment was pursued for three months; at the end of which time I had the happiness to find the pain was almost entirely subdued; the farther increase of the disease stopped; the quality of the discharge improved, and its fetor greatly corrected; and, although no diminution in size or number of the tubercles was perceptible, yet I was induced to hope that the case might be brought to a favourable termination, if, by any escharotic application, they could be effectually destroyed, continuing at the same time the use of the carbonate of iron. I first made use of the nitrate of silver. It occasioned great pain, and was inefficient in its operation. This was changed for the kali purum, which produced a rapid destruction of the diseased parts. The pain it gave was excessive; and, to prevent it, I latterly mixed a small portion of it with some hogs lard, covering the diseased surface with a plaster of it. By this mode of application the pain became considerably less intense, indeed very trifling, while the escharotic properties of the remedy were not in the least diminished; though it was necessary, in order to produce the same destruction of the parts, that the plaster should remain on twelve hours. It was requisite to use it only about one day in six. On the intermediate days, the carbonate of iron was applied night and morning. This was found frequent enough to keep the tubercles on a level with the general surface of the breast; and as soon as this change was effected, cicatrization commenced at the whole circumference of the sore, which continued in a progressive state of amendment for about four months, when I had the satisfaction to find both breasts perfectly healed, and the health of the patient completely reestablished. This was in December 1812. She resides at a considerable distance, but I occasionally hear of her, and she is at this time in perfect health.

During the progress of the case, I found it answer equally well,

well, if the kali purum cerate was applied on those tubercles only which formed the outer edges of the sore; for until their size was greatly diminished, cicatrization only took place at the circumference; and if the central tubercles were destroyed, they soon reappeared. She took the iron internally for the first three months, at the end of which period, by her own particular desire, it was discontinued; and as, after the change in the local treatment, the cure went on well without it, recourse was not had to it again. The annexed engraving, with the above description, will give a tolerably accurate idea of the appearance of this singular disease. I call it singular, as I have not been able, in the writings of Sir Everard Home, Mr Abernethy, or Mr Carmichael, to find a parallel case. By referring to these authors, I do not wish it to be understood that I consider this a case of genuine cancer; although most of its symptoms were common to that disease; but if Mr Carmichael be correct in considering lancinating pains as a certain pathognomonic sign of cancer, this, then, will certainly come under that appellation. On this point, however, Mr Abernethy and Mr Carmichael, it is well known, are at issue. *

Mr Carmichael states (page 342) that carbonate of iron gives no pain in cancerous ulcers; but that it does in those of every other description. No pain was produced at any time by its application in the case above related; on the contrary, the patient never failed to experience great ease after its use The fortunate result which was ultimately obtained, must be attributed to the combined operation of the two local remedies. For although the carbonate of iron prevented the extension of the disease, and apparently destroyed the diseased action, it was incapable of removing the tubercles; and, until they were destroyed, it was evident that little or no progress could be made in the cure. Their destruction was easily effected by the kali purum, while the form in which it was used, prevented in a great measure the pain and inconvenience

* "A lancinating pain in the part, frequently accompanies its growth: but, in some cases, this pain is wanting. It attends also on other tumours, the structure of which is unlike carcinoma; of which I have given an instance in speaking of pancreatic sarcoma. This therefore cannot be considered as an infallible criterion of the nature of the disease."—Abernethy on the Classification of Tumours, page 72.

"Mr Abernethy alone is of opinion that in some cases this distinctive mark of cancer is wanting; but in the great multitude of cases I have seen, there was not one in which this circumstance was not more or less evident, and I am persuaded that that intelligent practitioner must be mistaken in his observation."—Carmichael on the effect of Carbonate and other preparations of Iron upon Cancer, page 314, 315. 2d Edition.

inconvenience usually attendant on its application. It was farther evident, from the reproduction of the tubercles after they had been nearly destroyed by the caustic, that the kali purum alone would not have completed the cure; and I am disposed to believe, from what I observed in the progress of the case, that had not the carbonate of iron been applied at the same time, it would have occasioned a more rapid growth of the tubercles, and a consequent extension of the disease. The sores also remaining so long stationary before the kali purum was resorted to, is a sufficient proof that the cure would not have taken place so early, if at all, had the carbonate of iron alone been relied upon.

Poole, March 27, 1814.

IV.

Case of Ischuria Renalis. By Dr Laing, Fochabers.

ISCHURIA Renalis appears, by the concurring testimony of medical practitioners, to be a disease of very rare occurrence. Sir Gilbert Blane, in his Treatise on the Prevalence and Mortality of various Disorders, mentions, that only two cases of it had occurred to him in the course of his long and extensive practice. A case of suppression of urine, arising from affection of the kidneys, is recorded by Dr Home in his Clinical Experiments, in which the treatment was nearly similar to that adopted in the following one; but it seems, in that instance, to have been complicated with various other disorders of the vital organs, rendering the effects of the remedies uncertain, and leading to an inevitably fatal termination. It is hoped, therefore, that the following account of a very decided case of that disease, which lately occurred to me, may not be altogether uninteresting.

P. G. aged 72, was seized, about the 24th of September 1813, with pain in the abdomen, accompanied with feverish symptoms. The urine was lessened in quantity, and passed with difficulty. These symptoms rapidly increased. On the 28th, in the evening, when I first saw him, he had passed no urine for 48 hours. His pulse was about 90. There was no particular swelling observable above the pubes, nor any very urgent desire to make water. He only complained of restlessness, and general dull pain over the whole belly and back. As his bowels were costive, an ounce of castor oil was immediately given, and warm fomentations applied

to

to the abdomen. A draught, containing 25 drops of tinct. opii, and 40 drops of sp. æther. nitros. was directed to be taken after the operation of the cathartic. Next day, September 29th, he was nearly in the same state. The purgative had operated freely, but no urine had been passed. The belly appeared slightly distended, but no circumscribed tumour could be felt in the region of the bladder. It was difficult, however, to ascertain exactly the state of this organ, owing to the corpulent habit of the patient. I thought it proper therefore to introduce the catheter; but only a few drops of urine, tinged with blood, were discharged. The fomentations were ordered to be repeated frequently, and a clyster containing 40 drops of tinct. opii to be thrown up at night. He was also desired to drink freely of linseed tea; and to take a powder, consisting of nitre and powder of gum arabic, each ten grains, every four hours. Not being entirely satisfied with regard to the state of the bladder, as it appeared to me possible that the catheter might have never reached it, owing to some accident or peculiarity of conformation, I requested the attendance of a neighbouring medical gentleman next morning.

When we met on the 30th, the patient was still in the same state. The catheter was again introduced, and we were both satisfied that it had entered the bladder; but still no urine followed. His pulse was now 94, and he complained of much thirst, and dull pain through the whole belly and back. The nature of the complaint being now in some measure ascertained, we determined on trying the effect of moderate bleeding. Eighteen ounces of blood were accordingly taken from his arm. He seemed somewhat relieved by the operation, and the blood was slightly sizy. The remedies were ordered to be continued, and the warm bath at 98° to be applied for half an hour in the evening. The castor oil was also repeated in the morning.

Next day, October 1st, I did not see him; but on the 2d I found him considerably easier. The castor oil had produced many copious watery stools, with much relief to the distension of the abdomen, but still no urine had been passed. Eight ounces of blood were this day taken from his arm, and the warm bath repeated. Cream of tartar was substituted for the nitrous powders.

On the 3d, he was exactly as before. The blood had assumed a thick buffy coat. The medicines and warm bath were continued, and ʒx. of ol. ricini ordered to be given in the morning. Next day, the symptoms still continuing, ℥xv. of blood were taken from his arm, which produced considerable relief. The castor oil had had the same effect as formerly, in producing a copious watery

watery discharge, but no urine had appeared. All the remedies were continued.

On the 6th, I found him much better. A little urine had been passed while in the warm bath, on the evening of the 5th, and a few hours afterwards two ounces were passed spontaneously. It was of a natural appearance, but rather high-coloured. The pulse was now about 86, and the feverish symptoms much abated. From this time he continued to improve daily, and the quantity of urine gradually increased. The diuretics, laxatives, and warm bathing were continued; and he soon recovered his usual health. No dropsical swellings took place, and no calculous matter was voided, either during or after the complaint.

In this case, the functions of the kidneys appear to have been entirely suspended for nine or ten days without any material injury resulting to the rest of the system. During this period, the perspiration was not sensibly increased; and there was none of that urinous smell about the patient which frequently attends suppression of urine. The only evident outlet for the quantity of liquids introduced into the system, was the copious watery discharge produced by the purgatives. The immediate cause of these symptoms appears to have been an inflammatory affection of the kidneys, indicated by the pain in the back, feverish symptoms, and sizy blood; but differing materially from ordinary nephritis, in the absence of the acute pain, high fever, vomiting, and frequent painful micturition, which characterize that disease, and in the total suspension of the urinary secretion. On what this difference depended it is not easy to determine. Probably the membranous coverings of the kidney may be principally affected in acute nephritis, and its medullary substance in the present case. This would, in some measure, account for the more acute pain, fever, and vomiting, in the former case, and for the less urgent general symptoms, but more complete suspension of the functions of the organ in the latter.

Fochabers, June 26, 1814.

V.

Remarks on the Nature and Treatment of Puriform Ophthalmia. By William James Wilson, Fellow of the Royal College of Surgeons, London, and Surgeon in Manchester.

Few diseases, to which the organ of sight is liable, have a more direct tendency to effect its destruction, than that species

species of ophthalmia, which, from a remarkable symptom attendant on it, has been denominated puriform. Much praise is due to the late Mr Saunders, for a very accurate history of the disease; indeed, before his essay on the subject appeared, practitioners were not well acquainted with the nature of certain organic changes which take place when the complaint assumes a violent form. These changes are the effects of excessive action in the vessels of the conjunctiva, eventually extending to that part of the membrane which covers the front of the transparent cornea. In common ophthalmia, the disease is often confined to the conjunctiva of the palpebræ and sclerotica; and, even where it extends to the cornea, the inflammation, for the most part, terminates in the adhesive stage, and more or less opacity is the consequence. But, in cases of that form now under consideration, there is but little disposition to the effusion of coagulable lymph. It, however, sometimes takes place; and, provided the inflammation has begun to decline, may be looked upon as a favourable symptom. In extreme cases, the conjunctiva sloughs; and as that part of it which passes over, and is most firmly attached to the cornea, has but little power to withstand disease, a slough, or sloughing ulcer, first forms on the conjunctiva of the cornea. Eventually, the proper lamellæ of the cornea partake of the same morbid actions; but I believe it is only from contiguity that the actual substance of this tunic becomes affected, the conjunctiva being the primary seat of disease. In proof of this, we may observe, that the slough first formed is superficial, and soon cast off. Afterwards, if the disease is not checked, the sloughs are deeper, and more time is required for their separation. In no instance have I observed the anterior chamber to be opened at once; but where this unpleasant circumstance has happened, it has been by a *succession* of sloughs, or the spreading of ill-conditioned ulceration. The puriform ophthalmia of the infant, and that of the adult, often termed the Egyptian ophthalmia, produce the same effects on the conjunctiva and cornea. When the inflammatory symptoms run very high, sloughing is the consequence; when less violent in degree, an effusion of coagulable lymph may take place. The opacity produced by this effusion is to be carefully distinguished from a peculiar dusky appearance observable on the cornea, when a portion of it is about to slough; an appearance much to be dreaded. It is indeed, as a late writer has well expressed, the "unerring harbinger of mortification." When the disease has been neglected, or we have not been able to put a stop to the sloughing process, the anterior chamber of the aqueous humour becomes opened. The iris then protrudes, and, in its turn, ulcerates;

cerates; finally, the lens and vitreous humour issue through the breach in the cornea. This concludes the sad eventful history of a case of severe puriform ophthalmia. It fortunately happens, that such a termination is rare, compared with the frequency of the complaint. Even should the anterior chamber become opened, and the iris protrude, provided the aperture in the cornea is but small, and we pursue the proper treatment, the iris will adhere to the edges of the ulcer; lymph will be effused, and the healing process soon be completed. Though the iris will be for ever adherent to the cornea at that part, yet vision will not be materially impaired. It must be understood, however, that we can only hope for this favourable termination, when the circumstances I have mentioned occur in a slight degree; for, not to speak of the extensive opacity of the cornea, left on the healing of a large ulcer, the pupil will become so deranged, from being dragged under the opaque portion of the membrane to which it is adherent, as to prevent the passage of rays through it; nay, sometimes the pupillary edge of the iris will become fixed throughout its circumference, to a central penetrating ulcer of the cornea. Here it is obvious, that nothing will be likely to benefit the patient, but his submitting to an operation for cutting the iris, and removing the crystalline body, which in these cases I have always found opake. But this is a subject foreign to my present purpose. It is not my intention at this time to enter into a minute detail of the various measures to be adopted in the treatment of puriform ophthalmia. Indeed I wish to confine my observations to a consideration of the remedies to be used, where sloughs, or sloughing ulcers, have formed on the cornea. From the rapidity with which the disease runs through its course, those afflicted will frequently not apply for relief until the violence of the inflammation has begun to decline. It is about this period that the structure of the cornea suffers.

In every case of puriform ophthalmia, it is the duty of a surgeon to make himself acquainted with the actual state of the organ; and this object is frequently not very easy to accomplish, owing to the great tumefaction of the eye-lids. On an attempt being made to separate the palpebræ in the common way, the orbicularis muscle contracts, and projects the tumid conjunctiva, obscuring the whole ball of the eye. From having been witness to perhaps some of the worst cases that can possibly happen, I believe I am warranted in saying, that in no instance need we fail in our attempts to obtain a view of the cornea.

It is rather difficult at first to get the method of separating the eye-lids, so as to prevent the distended conjunctiva from protruding. Where difficulty occurs, I always use Pellier's speculum,

lum, which is, I think, preferable to any other instrument. On this subject I can speak with a good deal of confidence, having, whilst a pupil of the London Eye Infirmary, had daily opportunities of inspecting purulent eyes. I generally succeed best in the following manner: I first wipe the eye-lids from any discharge or moisture which may happen to be on them; and, placing the fore-finger of my right hand exactly on the edge of the tarsus of the upper lid, and my left thumb on the tarsus of the lower, I do not begin immediately to separate the palpebræ, but endeavour to get the swollen conjunctiva quite under the lids. Having done this, I press the tarsi upwards and downwards, fixing them completely on the bony margins of the orbit. By this means, the conjunctiva of the palpebræ is kept completely under the cartilaginous edges of the lids.

This is the way in which I make an examination with the fingers; but no general rule can well be given, as I imagine different surgeons may have different methods, equally good. Where chemosis is present, in any remarkable degree, the speculum is, perhaps, preferable to the fingers, as the discharge is apt to get under the latter, and cause them to slip. Notwithstanding all Mr Saunders has said relative to an inspection of the cornea, I am convinced that it is too much the custom to decline examination when any difficulty occurs. The complaints of a young patient, and the ill-timed interference of his friends, will also do much to deter a young practitioner. But when we consider how important it is, both with regard to our prognosis and treatment, no one will surely deny, that a knowledge of the precise state of the cornea is indispensably necessary. We may, indeed, sometimes *guess* that sloughs are forming on the cornea; for, in some particular habits, the general powers of the constitution become much depressed, the pulse is languid, and there is a tendency to profuse perspiration. We are not to wait for these symptoms before we examine, or to flatter ourselves, where they are absent, that all is going on well. I have selected the following case, from a vast number, as I think it shews several interesting facts in a very striking light:

Case. April 2d, 1814.—I was desired to visit George Jones, a weakly delicate boy, five years of age, and of a scrofulous habit. The disease had existed several days when I first saw him. I examined his eyes. The palpebræ were enormously swollen; puriform discharge very profuse. On the external margin of the cornea of the right eye was a dusky elevated opake spot, denoting that a slough had formed. The whole cornea was cloudy, but this latter appearance was totally different from

from that exhibited at the particular part I have mentioned. The conjunctiva of the left eye was most acutely inflamed; diffused opacity of the cornea, but no appearance of slough or ulcer. I injected a weak solution of alum on both eyes, and directed that it should be repeated every four hours; and, in the intervals, to keep the eye-lids moist with common saturnine wash. Four leeches were applied to the left eye.

℞ Ext. cinchonæ, gr. x. in pil. ij. dividend. 4tis horis sumend.

3d.—The sloughy spot of the right cornea was elevated, and had a groove around it; palpebræ less swollen. The bark was continued.

4th.—His pulse, which had hitherto been small and languid, had now acquired a good deal of firmness and regularity; puriform discharge lessened; palpebræ much less swollen. On injecting the right eye, I washed away the slough, which came off in one piece. The ulcer left by this separation has ragged edges, with a transparent bottom; the left cornea so opake the iris cannot be seen through it. The alum injection was omitted, as well as the bark. A purgative powder was given, and two leeches applied to the left eye-lid

5th.—The ulcer of the right cornea is extending; the discharge is of a light yellow colour, and more viscid; left cornea a good deal clearer. I injected a solution of alum on the ulcer of the right cornea, and again gave the bark in the same dose as before.

7th.—Lymph is deposited in the bottom and on the margin of the ulcer; the inflammation in the left eye has increased; the cornea is again so opake that the iris cannot be seen through it; discharge very profuse. A dose of calomel and jalap was ordered; four leeches applied to the left palpebræ; bark omitted.

8th.—Ulcer of the right cornea healing; left cornea rather clearer; tumefaction of the eye-lids and discharge abating.

10th.—Ulcer of cornea nearly healed; left cornea nearly clear, except a small central speck; bowels confined. Repeat the purge, and use a common saturnine wash.

12th.—Tumefaction so much subsided that he opens both eyes; his vision is tolerably good in the left eye; he marked out to me the figures on the face of a watch; there is a large opake spot on the outer margin of the right cornea left on the healing of the ulcer; discharge has not quite ceased. A weak solution of sulphat of zinc was ordered as a lotion. Acid. sulph. dil. gr. vi. ter de die.

18th.—The active inflammation has ceased in both eyes, but there is a profuse discharge of tears; they are morbidly sensible to the light.

22d.—I directed that a small quantity of a mixture, consisting of equal parts of vin. opii and water, should be dropped on the left cornea daily.

24th.—His general health is much improved; opacity of the left cornea diminished. Continue the acid twice a day.

My patient went on well till the 28th, when I was again desired to see him. I found that he had, the day before, been attacked by inflammation in both eyes; the right conjunctiva was very vascular, and discharged a puriform fluid; the left eye was but slightly inflamed; the palpebræ were beginning to swell. Three leeches were applied to each eye; a purgative was given; and I injected a solution of alum.

29th.—Right eye-lids enormously swollen; puriform discharge increased; right cornea quite opake; no slough or ulcer; left eye but slightly inflamed; complains of great pain in his head; pulse quick, with hot and dry skin. Two leeches to the right eye; a table-spoonful of common saline mixture, with four drops of tinct. digitalis every three hours. I injected the solution of alum.

30th.—The centre of the right cornea has become elevated; pain in the head very violent; stools offensive and dark-coloured; breath foul. A powder of calomel and rhubarb was ordered.

May 1st.—Central opake spot of the cornea is evidently a slough; at its upper part it is beginning to separate; there is also a dusky coloured spot on the cicatrix of the old ulcer; still complains of pain in his head; inflammation of the left eye ceased. I omitted the alum injection.

2d.—Nearly one half of the cornea has sloughed and separated; the bottom of the ulcer is also in a sloughy state; no headach; pulse feeble. Ext. cinchon, gr. x. 4tis horis. I injected a solution of alum on the ulcer.

3d.—Ulceration extending superficially along the cornea; the bottom of the ulcer is of a dirty yellow colour; he is much emaciated, and so weak he can scarcely stand.

5th.—The deep-seated slough at the bottom of the ulcer is separating.

7th.—The slough has separated, and the anterior chamber is opened; the iris is in contact with the edges of the ulcer of the cornea. Has continued his bark since the 2d; appetite improved, and he gains strength.

8th.—The iris protrudes; ulcer of the cornea very irritable. I injected a weak solution of nitrat of silver, and added acid. sulph. dil. gr. v. to be taken in a little water, with each dose of bark.

9th.—The portion of protruded iris has ulcerated; something came

came through the aperture in the cornea, which I took to be the lens in an opake state; the size of the eye-ball is much diminished; tumefaction of the palpebræ and discharge have quite ceased.

11th.—The parts are now taking on healthy actions; lymph is effused from the remaining part of the cornea.

12th.—Red vessels are shooting into the lymph.

15th.—The ulcer is closing fast. Since the 3d his bowels have been occasionally emptied by some mild aperient; the bark was omitted.

17th.—Ulcer nearly healed; the cornea is only transparent at its inner margin; has lost all useful vision in that eye; left eye continues well.

22d.—Ulcer is quite healed, and the strength of my patient being somewhat recruited, he left Manchester to enjoy the benefit of sea air.

Vehement local action often occurs, where the actual powers of the part and general constitution are but weak. In no instance is this more plainly shewn, than in some cases of sloughing of the cornea. The death of a portion of the cornea, in a young and tolerably healthy subject, takes place from violent inflammation, as may happen in any other part of the body. Here, generally, when the slough is cast out, the restorative process will go on uninterruptedly; but, in the scrofulous, or otherwise debilitated habit, it is widely different. The cornea is a part but feebly endowed with living power; a great effort of the constitution is therefore required to enable it to repair its diseases; so we find, that should the slough of the cornea, occurring in certain habits, become separated, the contiguous parts, participating in the morbid actions, also die. The healing of an ulcer of the cornea differs not from that of any other ulcer. Lymph must be effused; that lymph must be organized; the process of granulation must be completed. Do we not every day see this circumstance happen? An ulcer (let us say on the leg) is healing, granulations have shot forth, but the constitution, from some cause or other, becomes disturbed; suddenly the new formed parts are absorbed, and a deep foul ulcer is left. So again it is with the ulcer of the cornea. Where the constitution has begun to flag, I have seen an ulcer of the cornea, just on the point of healing, have its granulations all absorbed, or large portions of the membrane die, till the whole has been destroyed. In the case I have related, it will be observed that I gave bark in pretty large doses. I have, however, I hope, clearly defined the circumstances under which it was administered. The pulse was feeble; the sloughy spot on the cornea exhibited a well known appearance. I was convinced

that, when this slough separated, there would be little power to heal the ulcer. A spreading of this ulcer must therefore in all probability happen. At my third visit, finding my patient's general state so much improved, I omitted the bark, being unwilling to excite, even indirectly, too vigorous an action in parts already enfeebled by disease. I certainly expected that I should soon see the ulcer put on marks of healing. In this I was disappointed; for on the 5th the ulcer had an unpleasant appearance, and was extending rapidly. The bark was again given till the 7th, when, lymph being deposited round the margin, and at the bottom of the ulcer, it was a second time omitted. At the period when the tonic plan is to be adopted, the bowels ought to be regulated with the mildest laxatives, but purging is to be avoided. In the scrofulous subject, after the tumefaction and purulent discharge have ceased, and the ulcers healed, if there have been any, the complaint frequently assumes a chronic form; there is a profuse secretion of tears, and the eyes are morbidly sensible to the light; in short, a species of scrofulous ophthalmia succeeds the puriform. In these cases, I have seen the most decided benefit arise from the exhibition of small doses of calomel and opium combined. The constitution should also be invigorated by tonics, sea-air, &c. We must here be careful not to apply local stimulants too soon, even though there is no appearance of general increased vascular action in the organ. Here I have seen much mischief arise, from the indiscriminate use of the *vinum opii*; though there are cases requiring it. I am much inclined to attribute the second attack of inflammation in Jones's case, to the inadvertent application of a handkerchief to his eyes, which had been stained with the former purulent discharge; but I could never exactly ascertain the fact. The different stages of the disease were admirably shewn: 1st, On the left eye, the termination in the adhesive stage leaving opacity. 2dly, The striking difference in the appearances exhibited by this opacity, and the opake spot on the right cornea, previous to the formation of a slough. 3dly, The efficacy of bark in putting a stop to the sloughing; and though in the second attack it failed to do so, until the organ was almost destroyed, yet in the end it did accomplish the object for which it was administered. Had my principal view, in relating the case, been to confirm the fact that cinchona is the remedy on which we must often place our chief reliance in sloughing of the cornea, I could have brought a host of instances. Lastly, The process set up by nature to repair the disease, even when the whole substance of the cornea has been destroyed. Here there is an attempt to form a new cornea, but it is permanently opake.

Manchester, June 4, 1814.

VI.

VI.

Case of Hemiplegia, treated with Tinctura Lyttæ, and cold Shower-Bath. By ROBERT HARTTE, Surgeon to the Forces, Member of the Royal College of Surgeons, London.

HOPE, a private soldier in his Majesty's 1st West India Regiment, (while at Trinadad,) was admitted into the hospital on the 29th June 1812, with Cachexia Africana, for which bark, steel, and other tonic medicines were given. He appeared to be recovering; but, on the morning of the 18th July, when I visited my hospital, I found, that, from paralysis, he had lost the power of his right side; that he appeared to have no sense of feeling in it, or the extremities of that side; his articulation was scarcely to be understood; he seemingly did not suffer pain, yet he would frequently with his left hand take hold of his tongue, as if with a view of squeezing it smaller. Blisters were applied between the shoulders, along the vertebræ of the neck, as well as the region of the sacrum; asafœtida and other antispasmodics were employed without success. On the 29th July, I ordered the tinct. lyttæ to be given, five drops three times a-day, increasing the dose one drop daily.

On the 3d August, ten drops produced ardor urinæ: I discontinued the medicine for one day, and ordered rice-water for common drink, and the region of the bladder to be fomented. The following day, (5th August,) I again commenced the tincture with ten drops (ter in die) increased as before. On the 16th August, he was sensible when any part of his side or extremities was touched, and his articulation began to be more distinct; on the 20th August he could stand, with the assistance of a stick. I then ordered him to have the cold shower-bath every morning. 1st September. When at 37 drops per day, he could make some steps without the stick, I continued giving this quantity until the 20th September, when he was so convalescent, that I discontinued the tincture, and used the cold shower-bath only. 4th October he was discharged, and when I left the regiment, in 1813, he was in perfect health.

VII.

Case of Tænia in an Infant, cured by decoction of Pomegranate. By WILLIAM POLLOCK, M. D. Communicated by ADAM BURT, M. D. superintending surgeon, Bengal.

I WOULD have done myself the pleasure of transcribing for you *the case of tænia* in an infant, the moment I received your kind letter

letter, but my journal for 1811 had been mislaid, and I have only this morning discovered it. I formerly communicated to you some cases of tænia, cured by the pomegranate decoction, which were inserted by Dr Fleming in his Catalogue of Indian Medicinal Plants and Drugs, printed in 1810, and, since that period, I have found the remedy invariably successful, in a very great number of instances. In many of those the tænia had acquired an enormous length, and in some of them it was received in tepid water, and lived for several hours after it was passed. The following case occurring in an infant, not two months weaned, appeared to me to be very remarkable, in consequence of which I reported it to you, and also sent a copy of it to Dr Fleming, at the time it happened. That it is a very uncommon occurrence, may be inferred from an observation of Dr Hamilton's, in his most valuable work on purgative medicines, where he says, that the tænia is altogether unknown in infancy and childhood.

Peter Daly, aged 14 months. 27th August 1811. Was weaned about two months ago, and has since been gradually drooping, in consequence, as was supposed, of dentition. He is now excessively reduced, refuses all food, and is harassed with a constant troublesome diarrhœa. His skin is loose, dry, and shrivelled, and he has the whining fretful cry of a child who has been long sick. Different medicines have been prescribed for this diarrhœa without relief, and for several days his stools have contained small fleshy shreds, some of which considerably resemble half-dissolved portions of the tape-worm. Two ounces of water were added to six of a decoction of the pomegranate root, (prepared by boiling two ounces of its fresh bark in a pound of water to nine ounces,) and a table spoonful was ordered to be given the child every half hour, unless sickness and vomiting intervened.

28th.—He took the whole of his medicine yesterday, without either sickness or vomiting, and in the evening he passed a portion of tape-worm alive, upwards of six feet long. The medicine purged him briskly, and to-day he has vomited almost every thing he has taken. Appearing much exhausted, an anodyne carminative mixture was ordered to be given at intervals, to relieve the sickness.

29th.—Appears more lively; has had no vomiting since yesterday, and the diarrhœa has been much restrained by the anodyne. Quiescat.

1st September.—Is manifestly better in every respect, but his stools still contain portions of the tape-worm. Eight ounces of the above decoction, without dilution, were ordered to be given in the same manner as before.

2d.—He took the whole of his medicine yesterday, without either

either sickness or vomiting, and with but little effect upon his bowels, until this morning, when it began to purge him briskly, and he passed another portion of the tape-worm, nearly eight feet long. He has been very hungry, and ate a hearty breakfast.

25th.—No portions of the tape-worm have been observed since last report, and the diarrhœa gradually left him, without the use of any other medicine. His bowels have become regular, his appetite keen, and he has filled up apace. He has the appearance of a healthy thriving child, and his strength has improved so rapidly, that he has now begun to walk.

From the above period he continued to thrive until August 1812, when he again began to pass pieces of the tape-worm. The pomegranate decoction was repeated, and he passed an entire tænia alive, fifteen feet long, since which he has been in perfect health, and is at this moment a very fine boy. He is a son of Edward Daly, private soldier in his Majesty's 53d regiment.

Merat, 7th January 1814.

VIII.

Two Cases, with Observations, demonstrative of the Powers of Nature to reunite parts which have been, by accident, totally separated from the Animal System. By WILLIAM BALFOUR, M. D. Edinburgh.

THE practice of ingrafting trees first suggested to medical practitioners the idea of repairing mutilated parts. This practice was successfully prosecuted by a few, when the state of society afforded opportunities, but has uniformly been treated with a certain degree of ridicule, by far the greater number of the profession. This distrust in the powers of nature, in the face, too, of evidence which it would be difficult to controvert, is not more unphilosophical in itself, than its influence on practice must be detrimental. What a man believes impossible, he never will attempt. But if a branch of one tree unites with the stump of another, so as not only to live, but to bear fruit, it can be no great stretch of imagination to suppose, that a part totally separated from the animal system, may under proper management, reunite and live, and perform all its proper functions.

A number of years ago, an accident happened, in the management of which I proceeded upon the principle, of the possibility of parts entirely separated uniting again, with all the success I could desire. This idea was suggested by the necessity of doing something

something without delay, and the recollection of the Taliacotian mode of forming artificial noses. At that time, however, I knew of no case in which even an attempt had been made to replace parts, as near being totally separated from the system as were those in this instance. This case I did not publish at the time it occurred, for reasons hereafter to be mentioned. Indeed it had almost gone out of mind till a fresh accident occurred, the cure of which by reunion, ranks, under all the circumstances of the case, among the most wonderful instances of the powers of nature, and for ever sets at rest the question, "Whether parts, which have been completely separated from the rest of the animal system, and in which circulation has ceased altogether, can be again reunited?"* There is a circumstance, too, which stamps a value and importance on the two following cases, above all, or most others of the kind,—that is, their authenticity, or the proof that can be led, that the facts recorded really happened precisely as recorded; a proof, which, from the number and respectability of the witnesses, must convince the most sceptical.

The recollection of what took place in the first case, left little doubt in my mind of an equally favourable result in the second; for I never attributed the least degree of the success attending the former, to the small slip of skin that remained undivided. It appeared to me highly improbable, that a connection so very slender could keep up circulation betwixt the system and the separated parts, with a vigour sufficient to keep the latter alive.

From the moment, therefore, I found reunion to have taken place, I began to suspect, that the precautions employed by Taliacotius, and his contemporaries, to keep up circulation in parts destined to supply the place of others, were unnecessary, and inadequate to the purpose. Can it be supposed, for instance, that an extensive surface cut out of the arm for forming an artificial nose, could be fed by a small attachment, with a vigour at all proportioned to the celerity with which adhesion by the first intention takes place? It is impossible. Suppose a piece of skin, of such extent, were raised from any part of the body, and allowed to remain, without being connected with any other raw surface, how long, I ask, would that part of its margin, most distant from the attachment left to keep up circulation in the part, live? I am convinced very little, if any blood, would issue from it, except what might be in the vessels at the time of their being divided.

If, therefore, the connection left to keep up the circulation of the excised parts, in Taliacotius's operations, was insufficient for that

* Dr Thomson's Lectures on Inflammation, page 230.

that purpose, these parts must be considered as having been in the same situation as if entirely separated from the system, and their adhesion to the parts to which they were applied as having depended, not on the circulation supposed to have been kept up in them, but on the principle of vitality remaining in them, and on the circulation having been restored to them, by the new surface to which they were attached.

With regard to circulation being kept up, in parts cut out of the forehead, as now practised in India *, it is out of the question. The twist given to the small attachment, left at the root of the nose, must almost entirely preclude any thing of the kind.

I am convinced, therefore, that had Taliacotius at once separated from the system, the flaps of skin with which he repaired mutilated parts, his operations would have been equally successful, infinitely less troublesome to himself, and distressing to his patients. With this conviction, arising solely from the success attending the first of the following cases, I was resolved to attempt the reunion of any divided parts that might come in my way, unless such parts were of a magnitude that the apposition of the wounded surfaces would not restrain hæmorrhagy.

There are instances on record, with not one of which was I acquainted when the accident happened to my son, of the *points* of fingers, ears, noses, being nearly or entirely separated from the system, that were made to readhere. But Pedie's case, so far as I know, is without a parallel; and I have the authority of a number of the first characters in the profession in this place to say, that it is the most extraordinary that has come to their knowledge. This is my apology, if any is necessary, for giving it to the public. For "it must not be imagined, that the recital of such uncommon cases is without its use; for, while they extend our knowledge of the powers inherent in living bodies, they inform us of the advantage often to be derived from allowing these

* A thin plate of wax is fitted to the stump of the nose, so as to make a nose of a good appearance. It is then flattened, and laid over the forehead. A line is drawn round the wax, and the operator then dissects off as much skin as it covered, leaving undivided a small slip between the eyes. This slip preserves the circulation till an union has taken place between the new and old parts. The cicatrix of the stump of the nose is next pared off; and, immediately behind this raw part, an incision is made through the skin, which passes around both *alæ*, and goes along the upper lip. The skin is now brought down from the forehead, and, being twisted half round, its edge is inserted into this incision, so that a nose is formed with a double hold above, and, with its *alæ* and *septum* below, fixed in the incision. The connecting slips of skin are divided about the twenty-fifth day, when a little more dissection is necessary to improve the appearance of the new nose.—Gentleman's Magazine for October 1794.

these powers proper opportunities of exerting themselves. Of reunion, by adhesion, we are, in no case, to despair, so long as the least degree of circulation remains in both, or even in one of the parts divided *." To this I would add, that many things are left undone, from mere supineness, or a belief that they are impracticable, because they were never known to have been done before. Of what advantage then may it be, to be generally known, that such things as are detailed in the two following cases, are not only practicable, *but have been done?*

Individuals in every line of life, but especially those who work with edged instruments, or who are employed about machinery, would often have it in their power to prevent unsightly mutilations in themselves or others, and be enabled to earn their bread as before. Surgical skill is not necessary in all instances of such accidents. The chief thing wanted is, a conviction that attempts at reunion of divided parts *may* be successful; for whoever has this belief will not fail, when such accidents occur, to give opportunity to the powers of nature to exert themselves. There is, indeed, no saying what may be done in this way. It would be unphilosophical to set bounds to the powers of nature. And because noses, ears, and the *points* of toes and fingers, are the only parts which, when separated, are known to have reunited, is nature to be blamed for that? The reason is, the reunion of other parts has never been attempted. I would not, however, from these observations, be understood as imagining, that parts of very considerable magnitude, when totally separated from the system, can ever be expected to reunite. The impossibility of this is, I think, evident, *à priori.*

But the knowledge of such facts as occurred in the following cases, must, by increasing his confidence in the powers of nature, induce every rational practitioner to trust more to them, in many circumstances, than he otherwise would do. Therefore, though it would be madness to expect the reunion of a leg or an arm that had been entirely separated, yet I can very easily conceive a leg or an arm to be wounded in such a way, by accident, or in battle, as, according to received principles, to render amputation necessary, but which in the hands of a surgeon, quite aware what nature can do, might be preserved. I can very easily conceive that, to a practitioner who knows that nature, by her own innate powers, unaided by a single auxiliary artery, can effect the reunion of a finger that has been entirely separated from the system for nearly half an hour, cases may occur, in which he will pause before he takes off a limb, which, but for such knowledge, he would amputate

* Dr Thomson's Lectures on Inflammation, p. 243.

amputate without hesitation. In this view, Pedie's case in particular, will, I trust, be an acceptable present to surgeons in the army and navy, where casualties occur so often, and of course there are such opportunities for observation, and warrantable experiment, and may be the cause of preserving limbs which otherwise would be lost, to many a gallant man. At all events, it is evident, that the practice of attempting the reunion of separated parts, may be carried farther than has ever yet been done; and it must be a comfortable reflection to an unfortunate sufferer, in the hour of accident, to know, that a whole finger, or even all the fingers of a hand, though entirely cut off, may be restored. *

CASE I.

About eleven years ago, Mr Gordon, surgeon, now, I believe, in India, after having conversed with me for some time one day, in my shop, upon going out shut the door smartly after him, without perceiving any body near it. Unfortunately, one of my sons, a boy of about four years and a half, diverting himself on the outside, had one of his hands in the groove of the hinge side of the door. I was shocked with a wild scream that I heard upon the door being shut; and still more so, when Mr Gordon came in, carrying the boy in his hands, stretched, from agony, as upon a rack. The points of three of his fingers were completely separated, with the exception of a slight attachment of skin, which barely suspended the parts. The points hung at right angles when the fingers were extended. The point of the index was cut off at the middle of the nail, the fore-finger a little above the nail, and the ring-finger at the root of the nail. The wounded surfaces were necessarily much bruised, but the fingers were nevertheless, cut so perpendicularly, that, unless I had seen it, I could not have believed a door could have done it. With the assistance of Mr Gordon, the innocent cause of the accident, I instantly replaced the parts, with but little hopes, I confess, owing to the degree of contusion of the wounded surfaces, of reunion taking place. But I was so shocked at the idea of the boy's hand being mutilated for life, that I hesitated not a moment to put the powers of nature to the test. On the sixth day

* A gentleman, to whom I told Pedie's case lately, expressed his regret that he had not known it sooner; for, said he, "Our blacksmith, a very clever fellow, a short time ago, struck off three of his fingers, from about the middle. He ran immediately, with the pieces hanging by small slips of skin, to the surgeon of the village, who out with his scissars, divided the slips of skin, threw the fingers away, and contented himself with dressing the stumps." Now, this gentleman did what almost every other surgeon would have done; but had he known Pedie's case, the blacksmith, a thousand to one, would have had his fingers to-day.

day after the accident I removed the bandages, when I found adhesion had taken place, to the unspeakable joy of Mr Gordon, the boy, and myself. The skin and nails came off all the three fingers, but were afterwards renewed; and the cure was so complete, that a narrow inspection was necessary to discover any difference between the fingers of the one hand and those of the other. There was, indeed, no difference to be perceived, but a slight scar on the left side of the ring-finger, at the root of the nail. This case I certainly would have published at the time it occurred, but on Mr Gordon's account, who, though not the smallest blame was attributable to him, suffered more anxiety and distress of mind than I did myself, and never liked to hear the subject mentioned. I trust he will now excuse me for mentioning him by name, having no other motive for so doing, than the establishment, beyond the possibility of contradiction, of the truth and accuracy of the above statement. Mr John Moffat, accountant of Excise, Mr Alexander Milne, surgeon, now on board the Norge, 74, and my servants, were likewise witnesses of the facts. The boy died of the scarlet-fever, a year and a half after the accident; and but for the following case occurring, which to most, I am sensible, will appear much more interesting and decisive, that of my son would never have been recorded.

Case II.

On the 10th day of June last, two men came into my shop, about eleven o'clock forenoon, one of whom, George Pedie, a house-carpenter, had a handkerchief wrapped round his left hand, from which blood was dropping slowly. Upon uncovering the hand, I found one half of the index wanting. I asked him what had become of the amputated part. He told me had never looked after it, but believed it would be found where the accident happened. I immediately dispatched Thomas Robertson, the man that accompanied the patient, to search for and bring the piece. During his absence I examined the wound, and found that it began near the upper end of the second phalanx, on the thumb side, and terminated about the third phalanx on the opposite side. The amputated piece, as measured by the patient himself, was an inch and half long, on the thumb side, and an inch on the other. The wound was inflicted in the cleanest manner, by one stroke of a hatchet, and terminated in an acute point.

In about five minutes, as nearly as I can guess, Thomas Robertson returned with the piece of finger, which was white and cold; and I remarked to Dr Reid, who was present, that it looked and felt like a bit of candle. Without the loss of a moment,

I poured a stream of cold water on both wounded surfaces, to wash away the blood from the one, and any dirt that might be adhering from the other. I then applied, with as much accuracy as possible, the wounded surfaces to each other, expressing a confident expectation that reunion would take place.

I endeavoured to inspire the patient with the same hopes, by detailing to him the success I had in my son's case, which, for the reasons already mentioned, was to me quite decisive of the question, whether or not parts entirely separated from the system would reunite? All this was heard by the patient with a very apparent distrust. But I could do no more than tell him, that, if reunion did not take place, no harm could ensue from the attempt, and that, if it did, a great deformity would be prevented. I informed him, that unless pain or fetor, or both, should occur, I would not remove the bandages for a week at least; directed him to keep his fore arm slung, and not to think of any kind of work. At last he entered so far into my views as to promise punctual obedience. He called on me next day, when he felt no particular uneasiness, but remarked, that the wound had not altogether given over bleeding. Assuring him there was nothing in that, desired him to call on me every day; but did not see him again till the 4th of July! Concluding, from his absenting himself without assigning any reason, that he was one of those, too frequently to be met with in the lower ranks, who go from one medical man to another, just as their fancy strikes them, or as they happen to be advised by some of their foolish and ignorant neighbours, and whose ingratitude to any practitioner is in exact proportion to the good he does them, I suspected he had fallen into bad hands, and that I never would hear more of him. On the 2d of July, however, a gentleman called on me, and asked if I recollected a man who had got a finger struck off, about three weeks before, to have come through my hands?

I told him I recollected perfectly well; that I was filled with indignation at the fellow's unreasonable and ungrateful conduct; and that I was just about setting on foot a search after him, not having informed myself either of his name or place where he was employed, at the time he applied to me. The gentleman said he would save me the trouble, for he could give me an account of the man.

The accident happened on the 10th of June, and on the 12th, the patient, under the influence of the ridicule of his acquaintances, for giving the least credit to my assurances that reunion would take place, applied to another practitioner. This gentleman, I am informed, on being told the object I had in view in replacing the piece of finger, represented the impropriety of any

other

other person intermeddling with it. But, prepossessed with the belief that he carried about a piece of dead matter only, tied to the stump of his finger, the man insisted on having the bandages removed, which was done accordingly. Thus were nearly rendered abortive, my attempts at the reunion of the parts, and the profession deprived of a fact, which, as demonstrating the wonderful powers of nature to repair injuries, is inferior in importance, to none in the annals of the Healing Art. But, fortunately, nature had been too busy for even this early interference to defeat her purpose. ADHESION HAD TAKEN PLACE.

In consequence of the information I got from the gentleman who called on me on the 2d of July, I found out the patient on the 4th, when reunion of the parts was complete. The finger, in fact, is the handsomest the man has, and has recovered both heat and sensation. In the progress of the cure, the skin was changed, and, soon after the accident, the nail fell off; but I have not the smallest doubt that it will likewise be renewed.

From the information obtained, not only from the patient himself, but from those present when the accident happened, I am satisfied, that upwards of twenty minutes must have elapsed before the parts were replaced. For the patient did not apply to me *immediately* upon receiving the injury. He waited on the spot till a great number of his fellow workmen, separated in different apartments of a large building, came to see and condole with him on the occasion. The word *immediately*, in his affidavit, must therefore be understood as so qualified.

I have thought it proper to subjoin the affidavits of George Pedie, Thomas Robertson, and Dr Reid, to the principal facts and circumstances of Pedie's case, that no doubt might remain of their truth and accuracy. For "it must be confessed, that instances of reunion among parts which had been entirely separated, are very rare in the human body; so rare, indeed, that most practitioners still treat with disbelief and ridicule the few instances which have been put upon record." * These affidavits are still more necessary to convince people who are not of the medical profession, but to whom the knowledge and belief of such facts may be useful. Numbers of such having heard an imperfect account of Pedie's case, have called upon me to ascertain the truth; but I have never yet met with one who expected me to confirm the facts, of the *entire* separation and *complete reunion* of the parts.

I,

* Dr Thomson's Lectures on Inflammation, page 239.

I, GEORGE PEDIE, house-carpenter, declare, That, on the 10th day of June last, when at work in the Advocates' Library, I accidently struck off the finger next the thumb of my left hand, at one stroke, with a hatchet: That, accompanied by Thomas Robertson, foreman of the work, I immediately went to Dr Balfour, who, as soon as he saw what had happened, asked where the bit of finger was that had been struck off? That I said I did not know, but believed it would be found where the accident happened: That Dr Balfour requested Thomas Robertson to go and bring it as quickly as possible: That Thomas Robertson went and returned with it in about five minutes: That Dr Balfour immediately washed both it and my bleeding finger with cold water, and replaced the piece that had been struck off, and bound it up: That Dr Balfour said he expected it would adhere, because he had been successful in a similar case eleven years ago, having replaced three of one of his son's fingers that had been cut off by accident, and which completely united: That I had no reason to go to any other than Dr Balfour, but that I did not believe the part of my finger that had been cut off would reunite, and that I was laughed at by all my acquaintances for ever expecting that it would: That when the dressings were first removed, which was on the 12th of June, reunion of the parts was found to have taken place. And I declare, that the merit of the cure belongs exclusively to Dr Balfour. All which I declare to be truth.

(Signed) GEORGE PEDIE.
DUNCAN COWAN, J. P.

Edinburgh, 18th July 1814.

Edinburgh, 19th July 1814, Compeared Thomas Robertson, mentioned in the preceding declaration, who being examined, declares conform to the preceding witness *in omnibus*. And this is truth.

(Signed) THOMAS ROBERTSON.
DUNCAN COWAN, J. P.

I PETER REID, physician in Edinburgh, declare, that I was witness to the facts and conversation stated in the above declaration by George Pedie; that I have this day examined his finger, and find that complete reunion has taken place.

(Signed) PETER REID, M. D.
DUNCAN COWAN, J. P.

Sworn before me, at Edinburgh, July 26, 1814.

From the above details, many questions naturally arise, any one of which I am far from pretending satisfactorily to answer. It is impossible,

impossible, however, to dismiss such a subject without hazarding some observations.

What, then, is the process which nature follows in re-establishing a connection betwixt the animal system, and a part that has been entirely separated from it? It is agreed upon, as the result of observation, however inexplicable the facts may be, that when two recently divided surfaces, both of which are still connected with the system, are applied to each other, with a view to immediate adhesion, or reunion by the first intention, a layer of gluten is first interposed between them. It is reasonable to suppose, that both surfaces contribute equally to the formation of this layer, which, soon after, is seen to be penetrated with blood-vessels. These vessels, however, are not unconnected, in any stage of their existence, with the surfaces. They do not begin in the substance of the layer and advance to the surfaces. They begin at the surfaces and advance towards each other; or, more properly speaking, they are the blood-vessels which had been divided, now elongated, through the medium of the organizable fibrin, for the purpose of reuniting the parts. Not so with parts that have been entirely separated from the system, and in which circulation has ceased altogether. The fibrin, in this case, must be effused from one surface only, that connected with the system. The vessels of this only can be elongated; and those of the separated part must be nearly passive in the process of re-establishing circulation. The separated part must be considered in a state of suspended animation, still possessing excitability. When new blood comes in contact with the open mouths of its vessels, it is probable that it is absorbed by capillary attraction. This new blood, being the proper and natural stimulus to its own vessels, must excite the dormant vessels to action; and upon this action must depend the connection that is formed between them and the vessels projected from the living surface. Circulation between the surfaces being thus established, must be gradually extended through the whole part that had been separated, by the *vis a tergo*, and the action of its own vessels.

Analogous to this is what happens to persons who are recovered from drowning. Though in them all the functions are suspended, the body is not dead. The principle of vitality still remains. It is, indeed, difficult absolutely to say when this principle is extinguished; at least, nothing short of the formation of new combinations can warrant the conclusion. Thus, though the body may have been a considerable time immersed in water, and is to all appearance dead, if the circulation of the blood can be restored, by the gradual application of heat, friction, and artificial respiration, it becomes reanimated, and is restored to the exercise of all its functions.

IX.

IX.

Pathological and Practical Observations.

(Concluded from p. 380.)

WHILE blood-letting is employed in such diseased conditions of the constitution to restore the balance of circulation, it is highly necessary to assist its operation, by promoting the natural evacuations also. By so doing, we not only call forth the agency of the several excretories, in accomplishing the great work of purification, but we enable these organs to relieve themselves from accumulated or morbid secretions, which the constitutional disease so generally gives rise to. These morbid secretions are particularly to be met with in the course of the alimentary canal, where they impede the natural functions, produce local distress, and even exert a considerable influence, by means of some unexplained connection, upon distant and sympathizing parts.

It will be necessary to illustrate each of these points; and that I may not exhaust the patience of those who may accompany me through this long discussion, I shall endeavour to explain myself on this head as briefly as I can.

In attending to the effects of medicines which evacuate the stomach and bowels, we perceive some well-marked differences in their modes of operation.

Of emetics, some merely discharge the floating contents of the stomach; others cause it to throw off its mucous secretions. Purgatives exhibit a still greater variety, and are of three sorts,—some evacuating the fecal contents of the intestines, others acting on their exhalent arteries, and producing copious watery stools,—and a third class stimulating the mucous follicles, which so abundantly line the intestines, and causing them to expel the mucous matter they so copiously secrete. When the bowels are merely inactive, their secretions healthy, and no constitutional disease present, the simple aperients of the first class suffice to obviate costiveness, and prevent feculent accumulation.

The second are requisite, when, in addition to unloading the intestines, it is desirable to abate arterial action, or allay fever, by reducing the quantity of the circulating fluid; and the third are required, either when the mucous secretions are so morbid

as

as to give rise to disease, or where they are too copiously generated, in consequence of increased action in the vascular system. The particular medicines belonging to each class, will naturally suggest themselves to every practitioner; nor is it of importance to make any very accurate arrangement of them. The same medicines will act differently in different habits; and it is the effect on which attention must be fixed, rather than the means which accomplish it.

Two medicines only shall I particularize as deserving a more detailed discussion; and, in treating of them, I shall, if possible, contrive to embrace every topic connected with the present subject. The medicines I allude to, are antimony and calomel; the former an emetic, the latter a purgative,—in which character only I mean here to consider it.

Each of these is remarkable for promoting mucous discharges, and, though belonging to different classes of evacuants, there is a strong analogy between their respective operations.

What antimony is to the stomach, calomel is to the intestines. But the resemblance is still closer, for calomel is capable of acting on the stomach, and of exciting vomiting, while antimony is well known to prove frequently purgative. Thus we see, that, in fact, the difference between them is in degree, not in kind; and that it consists only in calomel being more purgative, antimony more emetic.

According to the effect required, therefore, we have it in our power to supply, by one, that property in which the other is deficient; and, in the combinations of both, we possess a means of effectually evacuating the whole alimentary canal.

When it is considered that diseases of repletion are by far the most numerous that the human body is liable to,—that the alimentary canal affords one of the most important outlets for discharging the redundancies of the system,—that it is also a principal one for getting rid of the excrementitious impurities with which, in such diseases, the blood is speedily adulterated,—and that the diseased secretions which accumulate within it are oftentimes a means of continuing, of complicating, and even of creating various diseases in different parts of the body, the value of such a remedy cannot fail to be duly appreciated. It remains for me to shew, that such morbid secretions do exist within the stomach and intestines, and that they do produce therein the effects now attributed to them, being the direct cause of some local complaints, while they beget also, by remote sympathies, diseases in distant parts. Such indeed is their influence in this respect, that they even re-act on the circulation, whose increased actions first gave rise to them, and cause it to continue appear-

ances similar to those which result from a state of plethora, after this has been sufficiently relieved by evacuation.

Of the existence of superabundant mucus in the stomach and intestines, during inflammatory complaints, sufficient proof will be afforded, merely by inspecting the discharges brought off by particular evacuants, or occasionally by the natural efforts. With respect to the stomach, this examination may mislead, if only superficial; for the mucus being clear and colourless, is not readily distinguishable from the watery fluid surrounding it. Its tenacity, however, affords a ready means of demonstrating its presence; for, if a rod or wire of any kind is passed through the liquor and elevated, it will raise from the watery fluid the mucus existing therein, and sufficiently manifest its dense and viscid nature. It is this mucus that is produced by increased arterial action, affecting the mucous glands of the stomach in common with all other parts. To it, and to the action which produces it, as superseding the healthy actions of these parts, do I attribute the incipient nausea of fever, and of constitutional inflammation; and its expulsion I deem important, both as removing an injurious accumulation, and as enabling the secreting vessels, thus disincumbered, to continue those efforts, whose direct tendency is to relieve the general circulation, however inadequate they may be, when unassisted, to accomplish this purpose. Similar secretions are going forward also at such times throughout the whole course of the intestinal canal; and are evidenced by the quantity of mucus which a dose of calomel or antimony, administered under these circumstances, uniformly expels. And to the power of these medicines, in dislodging such secretions, is owing much of the superior efficacy, so long and so justly attributed to them in the cure of diseases.

The want of sufficient attention being given to the peculiar effect produced by different purgatives, may perhaps suffice to account for the uncertainty and indecision which still prevail in their employment, notwithstanding the high authority which Dr Hamilton's admirable work on this subject must carry with it. If this mucous matter is recently formed, and in no great abundance, a common purgative of the drastic kind will suffice to remove it, together with all such feculent lodgements as may have taken place in the intestines. A source of injurious irritation is thus removed; the various secreting and excreting vessels are left free to perform their natural functions; and the progress of nature, in her efforts to restore health, goes forward uninterruptedly.

If the mucous secretions are of older formation, and consequently more viscid, more tenacious, and more difficultly expel-

led, the common purgative fails to give relief, and a doubt is cast on the propriety of employing it, and on the veracity of former reports. The error here, however, is in employing a purgative inadequate to producing the effect required; for, if a competent dose of calomel and antimony is combined therewith to supply its deficiencies, the result will rarely occasion disappointment.

Further, if saline purgatives are given with the expectation of cleansing the intestines, when loaded with mucous secretions, they will very imperfectly effect this purpose; while, if persevered in, when the constitutional evacuations they procure are no longer necessary, they may wantonly exhaust strength, or otherwise injure.

Even the powerful remedy which a combination of calomel, antimony, and drastic purgatives affords us, may chance to be misapplied; and, if given when the bowels are irritable, with little mucus present, it is very possible that distress and injury may arise in this way, and a useful remedy fall into unmerited disrepute. The occurrence, I believe, is rare, nor have I had occasion to witness any instance of such misapplication; but such might arise, and mislead a practitioner, not accustomed to close and accurate observation.

In fine, by neglecting to ascertain the nature of the evacuation required, and to apply, with accuracy, the remedy suited to procuring it, we must even run considerable risk of disappointment in the effects expected from purgative medicines, and may so misapply them, as really to do injury, even in cases where purging, judiciously regulated, is the best or only cure of the complaint.

The quantity of this mucus secreted in acute diseases is very considerable. It lines both the stomach and intestines, and causes many powerful medicines to pass through them without producing their ordinary effects; for, in consequence of the interposed mucus, the medicines come only imperfectly or not at all in contact with the living fibre, which alone they are capable of stimulating. It passes through, therefore, as if either the living fibre was torpid, or the medicine inert, when neither supposition is correct; and to mistake and accident we are occasionally indebted for illustrations of this subject, which perhaps regular practice would more slowly and imperfectly afford us. For the errors of dispensers, and the stupidity of patients, have not unfrequently afforded me instances of inordinate doses of purgative medicines being given with only moderate and salutary operation. A boy, eight years old, was attacked with scarlatina, with high inflammatory fever, and much redness of throat, threatening speedy

speedy ulceration. Purgatives were freely used, both drastic and saline. By mistake six powders were given, containing a drachm of rhubarb, ten grains of calomel, and six of antimonial powder, in eighteen hours, one at the end of every third, with the effect of only discharging much dark green slimy matter from the intestines, and of abating the general fever. Purgatives were in consequence thenceforward more freely employed, and the effect regarded more than the dose necessary for procuring it; and although the inflammatory fever run high, and was not allayed for many days, there did not occur a speck of ulceration on either tonsil. Neither did any of the ordinary sequelæ attend the disease, but the recovery was progressive and complete.

In the beginning of the present year, a female labouring under enteritis was bled and purged freely. Being a pauper, a dozen of purgative pills were ordered at once, on motives of economy. By mistake, a full dose was given along with a saline antimonial mixture, every four hours; and in sixteen hours she took a scruple of calomel, a scruple of extract of colocynth, and twelve grains of antimonial powder. Yet the effect was only salutary, and the stools merely such as it was every way desirable should be procured. We may hence infer the difficulty of establishing the precise doses of medicines to be administered, and must be conscious of the superior advantage of attending solely to the sensible operation, when this is capable of being ascertained, disregarding altogether the quantity of medicine necessary for effecting it. This is always possible with respect to purgative medicines, and to be accomplished by regular inspection of the alvine evacuations, without which the practitioner must remain in much doubt concerning some of the most important operations going forward within the body, and must labour under great disadvantages in accurately applying the remedies it is necessary to employ.

Disordered secretions, such as I have been noticing, are found to exist where no particular plethora or state of arterial action can be referred to as having given rise to them. These may either have remained after the subsidence of fever, or they may have arisen from local stimulation only,—a supposition by no means improbable, when it is considered how numerous and manifold are the articles of excitement which luxurious habits have introduced into our common sustenance. Be this as it may, such morbid secretions are very frequently found in the stomach, where they occasion a large proportion of gastric diseases.

I shall at present particularize one only, namely gastrodynia, which appears to me so clearly referable to this cause, and so successfully

successfully treated on this principle, that I have long ceased to entertain a doubt on the subject. In this disease opium is a remedy frequently resorted to, for the purpose of allaying the spasm, and relieving the pain; and, in obstinate cases, the quantity of laudanum occasionally required to allay a paroxysm is so enormous, as to induce, in the mind of the practitioner who employs it, a firm conviction, that the insensibility to ordinary doses is owing to the violence of the spasm, and that the excessive dose is actually necessary to preserve life. An old and experienced practitioner of this class once told me, with much self-complacency, that he had just given in full doses, quickly repeated, 260 drops of laudanum, to relieve pain of stomach. My views of the disease, I own, are not in unison with such practice; nor shall I be inclined to alter them, so long as the means which I employ in treating the complaint continue so uniformly successful as I have hitherto found them.

Conceiving the pain to proceed from a contractile effort of the stomach to throw off from its surface the mucus which offends it, I have for many years laid aside the use of opium and stimulants, which merely repress the effect, without at all removing its cause, and which even tend to add to this, by stimulating the glands to increased secretion of the offending mucus, and have trusted solely to such medicines as act by expelling that matter to whose presence I attribute the complaint. When pain is severe, it may no doubt be advisable to allay this by a dose of opium, or some other stimulant. Nor is any injury sustained thereby, provided the real disease is borne in mind, and properly treated. But unless the severity of pain is great, I own I should be averse to relieving it in this way, or by any means but a removal of the offending matter,—as the relief to pain consequent upon such evacuation may be relied on as announcing the radical cure of the complaint. In some hundred cases that I have now treated on these principles, and which, of course, have presented almost every variety of kind and degree, including the form of pyrosis, which Cullen has pronounced to be so obstinate, I have in no instance given a single grain of opium, or failed of giving decided relief. Almost the only medicine I employ is a purgative, composed of extract of colocynth, calomel, and antimonial powder, and a moderate dose, given twice in the week, seldom fails to cure the disease, ere half a dozen are thus taken. Adjuvants I seldom employ, but they may no doubt be occasionally serviceable,—magnesia, and alkalies, especially ammonia, will correct acidity, and relieve heartburn, and other dyspeptic symptoms may derive benefit from other treatment; but when the foregoing purgative has been a few times employed,

employed, I very generally find, that all dyspeptic symptoms disappear, and leave the patient free from complaint.

The disease of colic I believe to be precisely analogous to gastrodynia, both in its pathology and treatment; and to differ only in being more prone to pass into inflammation.

The remote sympathies which different parts of the body evince, under disordered conditions of the stomach and digestive organs, have often engaged the attention of practitioners, and been recorded by practical writers. In no one, however, is the profession so much indebted for illustrating this subject as to Mr Abernethy, whose treatise "On the Constitutional Origin and Treatment of Local Diseases," affords an unquestionable proof of his distinguished talents, and must ever be deemed a most valuable acquisition to medical science. My present intention is only to notice a few of such morbid sympathies as have occurred in the course of my own experience, and I do so, still with a reference to my fundamental purpose, and in order to establish the principles by which blood-letting should be regulated and assisted in the cure of diseases, by exposing some sources of deception, which, if not understood, might thwart its beneficial effects, and weaken the confidence that deserves to be placed in it.

I have mentioned, that in all complaints attended with fever or constitutional inflammation, the gastric and intestinal secretions are quickly increased. I have stated also, that accumulations of morbid secretion oftentimes takes place in the alimentary canal, of slow and gradual formation, and not referable, by any distinct or well-marked connection, to a state of generally increased arterial action. The former state may even be superinduced upon the latter; and thus an additional complication, both of diseased action and of diseased conditions, ensue.

A disease in which this morbid state of the secretion exerts considerable influence, is rheumatism; and to the neglect of attending sufficiently to this, may be attributed much of the contradictory opinions which even practical writers have advanced respecting this complaint, and of the apparently incongruous treatment they have recommended.

Some consider all rheumatic affections as inflammatory, and to be treated by evacuations. Others assert, that evacuations may be pursued to the injury of the constitution, and yet the rheumatism be not removed. These, unwilling to employ so active a remedy as blood-letting, in a case that may resist its efficacy, are inclined to trust rather to diaphoretics, opiates, and tonics, as a safer alternative, however tedious or imperfect the recovery which they obtain.

Now, that acute rheumatism is a highly inflammatory complaint, can hardly admit of a question; the degree of inflam-

matory

matory fever, and of strong arterial action that attends it, place this beyond a doubt. Though blood-letting, however, is the only remedy for speedily allaying the inordinate action of the arteries, and for arresting the progress of disorganization in the affected joints, yet it is true, that the pains do not always abate in proportion as fever is allayed, but continue to tease the patient, under the form of chronic rheumatism, long after the disease has ceased to be characterized by increased vascular action. In several such cases I have had evidence the most direct for assigning this continuance of disease to the influence of morbid secretions within the stomach and intestines, and, by the full effect of an antimonial emetic, have succeeded in dislodging from the stomach a load of dense, viscid, and opake mucus, followed by copious stools, dark, slimy, and fetid,—and this even when the most active purgatives had been assiduously employed for weeks preceding. The effect on the disease has been an immediate alleviation of pain, a restoration of sound and refreshing sleep, which the nocturnal paroxysms had long prevented, and a cessation of the copious perspirations, which had for many previous nights drenched the patient, without in the least alleviating his sufferings; and this latter circumstance, I think, tends to throw much light on the history of rheumatism, and to show that it is not by diaphoresis that remedies of this class so often afford relief, but by some operation connected therewith, and to whose efficacy this contingent operation is not even essential.

I shall now briefly notice a case or two, in order to confirm the observations I have just been making.

A female, 32 years of age, had for eleven years been a cripple from contractions of the joints, occasioned by severe rheumatism. For several years afterwards she was subject to habitual diarrhœa, and, during its continuance, was free from her rheumatic complaints. The diarrhœa ceased, the rheumatism returned, and, for nine weeks before I saw her, she had been in constant pain, and almost without rest.

She had pains of ancles, knees, hip-joints, and loins, a furred tongue, costive bowels, and pulse at 130. The heat of skin was temperate, and she was covered with a copious eruption of urticaria, which had appeared a few days preceding. By purgatives she was in a few days considerably relieved, but, though the other symptoms were gradually abating, the pulse could not be got under 126. Believing the cause of disease to be now more peculiarly in the stomach, I ordered an antimonial emetic. It operated with some severity, owing, as I conceive, to the tenacity with which the viscid mucus adhered to the stomach. She succeeded however in discharging

charging an immense load of dense, viscid, opake phlegm, and nearly at the same time passed several copious stools, most foul and fetid, although for a week she had been freely purged, almost every day, by means of calomel and extract of colocynth.

The stools I did not see, as their intolerable fetor required them to be speedily removed.

The effect of evacuating them, however, was almost instantaneous; for on that night she slept profoundly, and next morning was free from all pain, with a pulse full, soft, regular, and at 92. By correspondent treatment, successive returns of disease were afterwards speedily and effectually relieved; her health in a short time became materially improved; and even the diseased and crippled joints were eventually so much amended, as greatly to increase her powers both of locomotion and of personal exertion of every kind.

The next case which I have recorded, was of a corpulent plethoric man of about 40. He was attacked by acute lumbago, which soon yielded to blood-letting and purging. Incautious in the management of himself, he soon returned to me with severe rheumatism of the chronic kind, under which he had suffered for ten days. The pains affected successively every joint; they deprived him of all rest; and both his arms and legs were nearly powerless, insomuch as to render him extremely low-spirited, and apprehensive of losing the use of them altogether. His tongue was moist, and slightly furred; his skin temperate; and pulse soft, and under 90. His bowels had been freely purged; for the only relief he had obtained, while trusting to his own management, was from the use of a purgative mixture I had previously ordered for him. Having been copiously purged the day on which I saw him, I ordered him only a dose of calomel, antimonial powder and opium, to be taken at bed-time. No relief, however, was obtained.

Next day I inquired respecting the state of his stomach, which he assured me was perfectly well, and free from nausea or any uneasy sensation. I ordered him, notwithstanding, an antimonial emetic, which he reluctantly agreed to take, being much disposed to question its propriety. With its effects, however, he was afterwards perfectly satisfied; for, after its operation, he walked up stairs, with the perfect use of all his limbs,—an exertion which he was previously utterly incapable of; slept soundly; had no perspiration; and arose next morning without one uneasy feeling, save a sense of weakness which he experienced in the limbs that had been principally affected.

The matter rejected by vomiting was not transparent mucus; but, as in the former case, consisted of a dense, viscid, opake phlegm,

phlegm, in large quantity. Several foul and fetid stools, too, were parted with, both during and immediately after the vomiting. The appearance of the matter rejected by the stomach, was such as no one could suppose to be a recent or a healthy secretion; and the fact of so morbid a state of stomach existing, without its being evinced by any of the ordinary indications, either of uneasy sensation or of depraved appetite, is well worth attending to.

It is also well worthy of remark, that such copious fetid stools so immediately ensued upon the action of vomiting, as almost to preclude the idea of their being occasioned by a direct effect of the antimony on the bowels; and to induce a belief, that they were owing to a simultaneous effort of the intestines sympathetically taking place, at the moment the stomach was parting with its diseased secretions.

The connection between these several diseased secretions, and the rheumatic affection, in both the cases I have now reported, cannot, I conceive, well be doubted; and the well-marked effects of the evacuations should lead us, I think, to a more accurate conception of the mode in which alterative doses of antimony so frequently prove serviceable, in curing this disease.

In these cases relief might, and no doubt would have ensued on such alterative doses, persevered in for a sufficient length of time; but then the *modus operandi* must have remained involved in much obscurity, which, by the speedier operation, and more obvious effects of the emetic, seems happily dissipated.

The cases themselves, to which I could, if necessary, add several others I deem valuable, more from the light which they shed on the general pathology of the body, and especially on that of rheumatism, than from the specific practice employed.

Indeed, I should be even inclined to hesitate in recommending its general adoption, and am much disposed to doubt the propriety of resorting to such full vomiting, unless where milder means are found ineffectual.

In consequence of the stomach being so firmly coated with its viscid lining, large doses of tartarized antimony are required to excite full vomiting. From 12 to 16 grains are frequently necessary, though given in doses of from a grain to a grain and a half every ten minutes; and the operation, when at length excited, is often extremely severe. Notwithstanding the frequent evidence which I have had, therefore, of the speedy and complete effect of full vomiting in such complaints, I have not of late been in the habit of employing it, in consequence of the distress it occasions, but have trusted rather to small doses of tartarized antimony given at some hours interval, and so increased, as at length

to

to excite either vomiting or purging. The operation of vomiting, when thus induced, is always mild; the stomach gradually throwing off its mucous lining, which, when once lodged in its cavity, a gentle effort suffices to expel, without either severe straining, or continued sickness ensuing.

Whether in all cases this gentle operation would be equally effectual as full vomiting, in dislodging such morbid matter as was discharged in the two foregoing cases, I am not prepared to pronounce. Whether there are cases, wherein the fuller operation, notwithstanding all the suffering that attends it, may not be preferable, in order to give more speedy relief to contracted or paralysed limbs, or to arrest the progress of disorganization, is what time and experience only can determine. It must occur to all who read these remarks, that many which are here applied to rheumatism are equally applicable to the kindred disease of gout, and that, connecting them with what has been so recently advanced respecting the state of plethora in which this disease originates, they tend still further to illustrate its pathology, and to point out the practice necessary for its relief. They assist, too, in explaining much of that extraordinary success, which has occasionally attended the exhibition of the celebrated *eau medicinale*; for, if the local inflammation which occurs in gout and rheumatism, is so intimately connected with the presence of morbid secretion in the alimentary canal, it is not surprising that so powerful an evacuant as the *eau medicinale* should remove the local complaint as speedily as the antimonial emetic did the rheumatic affections in the foregoing cases. Admitting, then, the pathology to be correct, which attributes gout to the existence of a state of plethora and inflammatory action in the blood-vessels, and the influence of vitiated secretions within the alimentary canal (which latter may be regarded in a great degree as the natural product of the former,) does it not seem to be fully within our power to bring this hitherto inveterate disease under the control of rational practice? and may we not hope to treat it as effectually, and much more safely, by the well-ascertained powers of such a remedy as a combination of colocynth, calomel, and antimony presents us with, as by the less manageable means of white hellebore, or the precarious and uncertain *eau medicinale?*

And this valuable form of purgative medicine I have frequently had occasion to employ with gouty patients, whose previous attacks had been left to nature, and who have been agreeably surprised to find, that, after the evacuations procured by it, the gouty attack subsided without those reiterations they had formerly experienced. That a remedy is thus discovered for gout, I by no means hold forth; for it is utterly inconsistent with my views

of

of the disease to conceive that any such can ever have existence. Believing, as I do, that it is caused, not by any specific virus, or morbid poison, capable of being neutralized by an antidote, but by a series of natural processes originating in a state of plethora, I can form no conception of any remedy admitting of indiscriminate application, or that can be suited to all the varied states in which this disease is presented to us. The means I would recommend are advocated, not from their possessing any secret or unexplained powers over the disease, but from their being pointed out by a rational pathology, and fully established, both with respect to their safety and efficacy, by extensive experience. For there is no period of medical history, which does not afford us ample evidence of such treatment having been successfully pursued; although prevailing prejudices, and the strong tendency which gouty patients so generally evince to yield to any delusion which affects to relieve their complaints without debarring them from the excesses which produced these, have ever given preponderance to the opposite system. To those who are sceptical on this point, I would recommend the perusal of Mr Ring's late excellent treatise on gout, which reviews the treatment of the disease from the earliest writers to the present day, and satisfactorily exhibits a host of authorities, fully sufficient to uphold any practitioner in the employment of evacuations, whenever his own judgment shall direct him to prescribe them.

No apology, I trust, is necessary for the minuteness with which this latter part of my subject has been treated, nor for the digressions into which the wish of fully illustrating it may have led me.

Bath. E. Barlow, M.D.

Analysis of the preceding Essay.

Prefatory remarks—Outline of the animal economy.

Process of digestion.

Deficient nutrition—from want of food—inability to receive it—impaired digestion.

Excessive Nutrition.

Pulmonary circulation; its effect in perfecting the blood formed by the chyle. Aortal circulation, and the distribution of blood for the purposes of life. Absorption of effete matter, and its return into the circulation. Excretion by the skin, intestines, kidneys, liver.

The foregoing animal processes reconsidered with a view to their classification.

Divided into the five following:

First Class.—Process of nutrition, as performed by the digestive organs.

Second Class.—Process of sanguification as perfected by the pulmonary circulation.

Third Class.—Processes of secretion, as performed in the course of the aortal circulation, by the several secretory organs, both nutrient and glandular.

Fourth Class.—Absorption, as performed by the lymphatics.

Fifth Class.—Excretion, as accomplished by the several emunctories.

Illustrations of the First Class.

Effects of repletion—Natural efforts excited thereby, and their tendency to remove it—When inadequate, disease induced.

Illustrations of the Second Class.

Action of the lungs excited by a twofold stimulus, namely, of the air, and of the blood—the latter deemed the principal one in inducing disease.

Illustrations of the Third Class.

Arteries excited by arterial blood—also by diffusible stimulants received into the stomach; the effects of which, however, in producing disease, are in a great measure dependent on the degree of repletion existing.

Illustrations of the Fourth Class.

Absorbents excited at their extremities by the effete matter of the system, and, throughout their course, by their lymphatic contents. The power of direct or specific stimulants on them questionable.

Illustrations of the Fifth Class.

Excretory organs stimulated by suitable blood—Their actions promoted also by exercise.

Analysis of the Second Part.

Introductory remarks—Apology for considering nervous influence as a secretion, and for referring it to the third class of animal processes.

Pathological principles, as deduced from the foregoing physiology, and from the phenomena of diseases.

Incipient

Incipient stages of disease introductory to a state of fever or inflammation.

Symptoms whereby they are characterized.

Transition into fever or inflammation.

Effects of depletion in relieving the incipient stages, with some rules for regulating the employment of blood-letting.

Auxiliary means to be employed in aid of blood-letting.

Sympathetic influence of morbid secretions found in the alimentary canal.

X.

Additional Facts and Observations relative to the Puerperal Fever which appeared at Sunderland and several places in 1813. By JOHN ARMSTRONG, M. D. Sunderland.

HAVING received, since the publication of my little work on puerperal fever, some valuable communications, which not only tend to throw useful light on the treatment of the disease which I have attempted to describe, but also to prove that it was really an epidemic of a truly dangerous nature, I feel it a duty, which I owe to the profession and the public, to give some account of these communications, connected with a few additional remarks.

In the winter of 1813, some cases of what may be termed puerperal peritonitis, occurred in the practice of Mr Gregson of this place; but they were not so serious as to excite much alarm, yielding to early and moderate depletion, with a very spare diet. As the year advanced, however, the disorder became more prevalent, and put on a more complicated character. Mr Gregson, in little more than a week, lost four patients, and Mr Stothard one, nearly about the same time. In the summer this disease continued to shew itself more frequently in the sphere of Mr Gregson's observation, but finally disappeared from Sunderland and the vicinity in October. During the same term, Mr Wolfe of Chester-le-Street, whose practice extends within a few miles of Sunderland, met with several cases of puerperal fever, but somewhat of a milder type than those above mentioned.

Nor

Nor was the disease restrained within these limits. In the beginning of May, it arose in the neighbourhood of Alnwick, and prevailed to a considerable extent there and at other places in Northumberland till the ensuing December, when it disappeared from that county also, after having occasioned a very considerable alarm. The sum total of deaths by this distemper, I have not been able to ascertain, but it must have been considerable, as Dr Ramsay informed me one accoucheur lost seven patients, in the immediate vicinity of Newcastle upon Tyne.*

This disease was varied in its appearance during its rise and progress, but it either put on the leading signs of what has been called the low puerperal fever, or those of the simple puerperal peritonitis. In the former, the symptoms of abdominal inflammation could scarcely be detected at a very early period, though accompanied by all the languor and oppression of typhus, together with a peculiar expression of the countenance which I have not witnessed in any other disease. In the latter, the fever was *apparently* of a more inflammatory character in the beginning, yet, when it continued two or three days, it bore a striking resemblance to the first mentioned.

It may not be irrelevant to remark, that this distemper was not connected in its origin with any morbid alteration of the lochial discharge, which indeed was not in any case affected till the full developement of the fever; after which, in by far the greater number of instances which took place at Sunderland, it was gradually diminished or suppressed. But, in the numerous cases which appeared in the practice of a most respectable accoucheur, Mr Wilson of Alnwick, † the lochia were at no period either diminished or suppressed, nor in any manner influenced till near the conclusion of the disease; resembling, in this respect, the puerperal fever which Leake has described, who expressly declares, that, in the cases which he has witnessed, the lochia were not obstructed, nor deficient in quantity. ‡ But generally, however, the lochial discharge, as in almost every other fever occurring in

* Documents are now in my possession, which prove that the same disease prevailed in 1811 and 1812, at two or three places in the county of Durham, and destroyed several patients.

† I avail myself of this opportunity of publicly returning my sincere thanks to Mr Wilson, for the most interesting account which he kindly transmitted to me, respecting the Northumberland puerperal fever. And it gives me great satisfaction to find, that the practice, which I have recommended, has the full support of this gentleman, whose extensive experience in the fever in question, and whose acute discernment, add a sterling value to his opinion.

‡ See Practical Observations on the Child-bed Fever, &c. By John Leake, M. D. 5th edit. Vol. II. p. 54.

in the puerperal state, is diminished or suppressed in the progress of the disorder under discussion; but, whenever this is the case, its diminution or suppression is merely a consequence, and not the cause of the fever. For my own part, and I have been consulted in a great variety of puerperal cases of one kind or other, I have never been able distinctly to trace the origin of any febrile affection to the diminution or suppression of the lochia, which have always appeared to me as the mere effects, rather than the causes of fever of any description, though some writers strongly insist upon a contrary view of this subject. Be this as it may in other febrile distempers, it must be apparent, from what has been advanced, that the lochia had not the smallest concern in producing the puerperal fever which I have described.

It is a singular fact, that in whatever place the fever in question occurred, it was principally limited to the practice of one accoucheur in that place. To adduce an example in point, Mr Gregson attended, with three solitary exceptions, all the women who were afflicted with the puerperal fever at Sunderland; and that gentleman has, with a liberality which does him the greatest credit, declared, that in his practice the fever was excited and kept up by contagion; and it seems utterly impossible to explain its frequent occurrence upon any other principle.

Wherever this disease, if I am rightly informed, was decisively treated by copious depletion at the very onset, it most frequently gave way, and was ultimately subdued; but, wherever a mild or a palliative treatment was pursued, or the fever far advanced before assistance was sought for, the event was generally mortal. In those cases which were treated at Sunderland by copious bleeding and purging, the recovery was rapid and complete; but in the few instances which were treated by purging alone, the recovery was more slow and doubtful, and there was a strong tendency to hectic some time afterwards, to which, indeed, some patients finally fell victims. Whether any thing similar was observed at other places, I have yet to learn; but shall not fail to communicate the result of my inquiries on this point, if at all of practical importance.

The foregoing statement of facts seems clearly to prove, that what have been termed the low puerperal fever, and the puerperal peritonitis, are merely modifications of one and the same disease, requiring the same, or a similar mode of treatment, according to variation of circumstances. The same principle of early depletion extends to both; but, of course, the successful application of this principle will depend much upon the choice of the means by which the evacuations are to be induced. At first sight it may appear, that the low puerperal fever should be treated

treated more mildly than the simple puerperal peritonitis; but this is truly a most fatal delusion; and I do not hesitate to affirm, that the former requires full as much depletion as the latter, with this important caution, that it is more pressingly demanded at the commencement, because the low puerperal fever is the more aggravated form of the disease, and because it is more rapid in its progress, and more destructive in its consequences than the other. Unfortunately for suffering humanity, the greater degree of abdominal inflammation, which *really*, though not always *apparently*, attends it, has been too frequently allowed to proceed uninterruptedly, under the fallacious mask of typhous languor and oppression. Most assuredly, this low puerperal fever, this terror of timorous minds, resembles many other fevers complicated with latent visceral inflammation; and, like them also, is only to be successfully encountered by early and active depletion. It was those medical phantoms, debility and malignity, which once deterred us from purging in typhus, and from bleeding in the bilious remittent fever of warm climates, and which are still conjured up to paralyze our efforts when they are most imperiously called for,—when we behold the very citadel of life attacked at once by the most destructive of diseases.

Dr John Ramsbotham of London has honoured me with a most important letter on puerperal fever, for which I here beg leave to offer him my best acknowledgments. That gentleman has seen both epidemic and sporadic cases, and is inclined to think, with me, that the low puerperal fever, and the puerperal peritonitis, are merely modifications of the same disease; but, having seen many women labouring under the latter affection, exposed apparently to all the circumstances under which contagion is usually generated, and not having observed it to spread beyond those individual examples, he has been led to suppose, that, in the epidemic cases, something is superadded to the abdominal inflammation, which renders them contagious. Upon this point he differs from the doctrines laid down in my recent publication, where it is stated, that both the low puerperal fever, and the puerperal peritonitis, are equally contagious in kind, though not in degree. If, from the facts which have come within my observation, I have drawn too general a conclusion as to the contagious nature of the disease, the above judicious remarks of Dr Ramsbotham will tend to rectify the error; but, at the same time, let accoucheurs be most guarded in their conduct, till this matter be established beyond the possibility of doubt. It may be, that the puerperal, like the yellow fever, is sometimes contagious, and sometimes not so; but, until we have

as satisfactory evidence upon the one subject as the other, I shall not fail, from what I have already seen, to warn my professional friends to act at least, whatever they may think, as if all the modifications of puerperal fever were really infectious.

It is most pleasing to me to find, that Dr Ramsbotham has preceded me in the depletory practice, both in the low puerperal fever, and the puerperal peritonitis; and he concludes, from considerable experience, that it is most certainly applicable to each in their commencement, according to the violence of the symptoms, and other circumstances. In the communication with which this excellent practitioner has favoured me, there is a more chronic species of puerperal fever noticed in a very particular manner, and which shall be here described nearly in the words of the author, as I have only barely hinted at it in my recent publication.

In this more chronic species of puerperal fever, Dr Ramsbotham observes, the symptoms proceed more insidiously than in any other, yet with equal danger. The pulse, at the commencement, ranges from 100 to 120 in the minute; there at first is little affection of the tongue, little affection of the belly, and that only complained of when the hand is pressed forcibly upon it for some time. The mind is perfectly collected, and the countenance for some days not altered; but the symptoms progressively advance day after day; the pulse gradually acquires a greater velocity; the pain of the belly increases; and, after some abdominal tension has continued for a few days, the mind begins to wander, the countenance to change, and death, too frequently, closes the scene. So secret are the underminings of this form of puerperal fever, that it, in general, is considerably advanced toward, or even into its second stage, before it can be satisfactorily detected. Such is the disease which Dr Ramsbotham has observed, and in which he conceives that the evacuating plan, by bleeding and purging, cannot be carried, even in its very commencement, to the same extent, as under a more acute and active state of the disease. Upon the whole, Dr Ramsbotham seems to consider this modification of the disease more dangerous than any other, inasmuch as it is not so readily recognized, and as prompt and active means, from the apparent mildness of the symptoms, do not appear to be so peremptorily demanded. About a month ago, I attended, with a surgeon of this place, a case answering exactly to the description above given, and it terminated fatally in the third week from its commencement; but, in it, advice was not obtained till near the accession of the second stage. Occasionally, a solitary case of this kind has come under my care, but generally at so late a period

riod as to preclude any thing like active treatment. From the few observations which I have been enabled to make, it strikes me, that this disease may, by minute investigation, sometimes be detected sufficiently early to justify depletion. The symptoms, upon which a diagnosis is to be founded, are, agreeably to my inquiries, the following: Paleness of the face; languor; listlessness; quickness of the pulse; uneasy sensation, not of pain, but rather of weight at the pit of the stomach; occasional involuntary sighs; flatulence of the stomach; pain of the belly, on forcible pressure; and slight chilly and hot fits, now and then, throughout the day. Probably one moderate venesection, carried to syncope, by bleeding the patient standing erect upon the feet, or sitting upon the breech with the trunk erect,* would be advisable in the beginning, together with the active employment of purgative medicines; but, respecting the treatment of this chronic form of the disease, it does not become me to speak with confidence, and I shall, therefore, earnestly recommend it to the consideration of future inquirers. In the low acute puerperal fever, however, and the puerperal peritonitis, no doubt exists on my mind as to the plan of cure which should be adopted; and I have adduced some evidence, which, it is more especially hoped, may not be disregarded by those men who have publicly recommended a stimulant mode of treatment in the epidemic modification of the disease.

Riverius and Baglivi have observed, that what are commonly designated low malignant fevers, are generally connected with visceral inflammation, which necessarily leads to fatal consequences when timely depletion is neglected. The experience of every day tends more and more to confirm me in the accuracy and importance of the above remark; having found fever, of whatever kind, *generally* connected, either in the relation of cause or of effect, with local inflammation. It is in the beginning of acute violent fevers, perhaps not excepting even the plague itself, that time is of such inestimable value; for there are then truly critical moments, which, if promptly seized, and judiciously employed, enable the practitioner to control the most untoward symptoms, and to prove that the medical art, with all its imperfections, is possessed of the most extraordinary powers. But if, by any chance, this golden opportunity be lost, the best directed efforts will generally fail to check the ravages of such disorders, which gather force as they proceed, and soon become uncontrollable,

* Patients faint under a much smaller abstraction of blood, when the operation is thus performed, than in the usual manner of doing it, in the recumbent posture.

uncontrollable, by causing irreparable derangements of function and structure. If, in the low puerperal fever, depletion has not hitherto been generally attended with the desired success, it is because that it has either been too late employed, or, when employed in proper time, has not been carried sufficiently far. It pains me, beyond expression, to think, that there are some distinguished men, who profess that nothing can be done in the low epidemic puerperal fever, but to palliate the symptoms; that, in reality, it is beyond the reach of the medical art. Surely, it would be far better in such men at once candidly to confess their ignorance on this particular disease, than thus to cling to the weakest and worst prejudices of times far less favoured than the present; than thus confidently to make declarations, which are calculated to deaden the best sensibilities of our nature, and to extinguish that ardour of investigation, which this, like many other subjects in medicine, so imperiously demands. As moral and responsible agents, we shudder at the bare imagination of coldly consigning patients to the grave without an effort to save them; and it is utterly unworthy of the age in which we live, to profess that the violence of any disorder is a sufficient reason for not attempting effectually to impede its progress. But, happily, the plain truth is, that no species of puerperal fever is absolutely incurable; on the contrary, if evacuations only be made at an early, a very early period of the first stage, and carried to a proper extent afterwards, the disease, under every variety of form, will not unfrequently be subdued. These are not vague assertions, prompted by the spirit of controversy, but they are truly deliberate opinions, founded upon cautious observations, and repeated experiments, and only thus earnestly offered to the world from an imperious sense of duty. Ill fitted, indeed, should I consider myself for the exercise of professional functions, if I were to allow myself to be deterred, through the fear of incurring the sarcasm of the critic, or the censure of the more grave professor, from the firm avowal of practical truths, how discrepant soever to the doctrines of the schools, or the speculations of the closet.

Wearmouth Walk, Sunderland, August 1814.

XI.

Observations on Pemphigus. By Dr Dickson, Physician and Inspector of the Fleet, &c. on the North American Station.

AS Pemphigus is a disease which rarely appears in Great Britain, I am induced to give some account of four cases which occurred in the hospital-ships in the River Medway, lately under my superintendence.

Case I.

G. Michaloff, ætatis 56, belonging to the Russian fleet, was admitted into the Argonaut, hospital-ship, on the 19th November 1813, in consequence of a vesicular eruption, of three days standing, which occupied almost every part of the body, but particularly the face, arms, and thighs. The vesicles were largest upon the thighs; but they were of every size, from a pea to that of a walnut, irregularly circumscribed, but generally oblong, and filled with a straw-coloured transparent fluid. Some of them were broken, and had discharged a large quantity of serum. The pulse was 110, the heat somewhat increased, but the tongue moist and clean. He was directed to take a purgative, and to be put into the tepid bath. To avoid repetition, I shall confine my observations chiefly to the progress of the eruption, and content myself with remarking generally, that the febrile symptoms were mild, and the functions nearly natural; and that the only treatment indicated throughout the disease, consisted of the occasional exhibition of a purgative, and some saline antimonial medicine, with a wash for his eyes; and mild dressings, after bathing the excoriated parts in tepid water.

20th.—Many of the old vesicles are broken, and several new ones have appeared. There are some of a small size upon the eye-lids and tarsi, and the eye-lids are inflamed. He complains only of the soreness of the excoriated parts; appetite and spirits good.

21st.—The vesicles on the eye-lids are ruptured; the tunica conjunctiva is much inflamed, and the exposure to light causes a copious flow of scalding tears.

22d.—As the old eruptions burst, new ones appear in various parts of the body. The ophthalmy has increased, the tarsi are swollen and ulcerated, and there is a copious agglutinating discharge which prevents the opening of the eye-lids, until bathed with milk and water.

23d.—

23d.—Few new vesicles have occurred since yesterday. He feels very sore; and the excoriated places secrete a copious purulent discharge.

24th.—The eye-lids are closed, and swollen, and there is a considerable puriform discharge; but no farther vesications have appeared. After this date no new vesicles formed; and the sores, after discharging considerably, dried up by degrees, leaving the skin of a deep red colour. On the 1st January 1814 he was discharged; but was sent back again to the hospital-ship, in about a fortnight, in consequence of several small blisters having risen on the scrotum and penis. They were not, however, larger than a pea, and soon healed; and no new vesicles having appeared on any other part, he was, after some time, finally dismissed. The skin retained a mottled appearance, from deep red patches of various dimensions.

He was not sensible of any indisposition before the eruption took place. He stated that he had been affected with the same complaint sixteen years ago, but was not aware of any cause, nor of having seen any one else with the same disease.

Case II.

J. Plusnin, ætatis 39, also a Russian, of a spare habit, was received into the Argonaut, on the 23d of November 1813, labouring under fever, from which he was nearly recovered, when, on the 1st of January 1814, some small vesicles were observed at the bend of the arm, where venesection had been performed. The pulse, skin, and tongue, were nearly natural.

2d.—He yesterday took a purgative. Some eruptions have appeared farther up the arm, and upon other parts of the body, which, on breaking, discharge a lemon-coloured serum.

6th.—Since the 2d, vesicles have continued to appear, and they are now general over the body, and upon the head, where they are very large. On the body, though most frequently the magnitude of a nut, they are of every size, from a pea to a walnut. The eyes, as in the case preceding, are much inflamed.

10th.—The hairy scalp is almost covered with large bullæ, which bursting, discharge an astonishing quantity of glutinous fluid, forming, when dry, an incrustation very difficult to remove. There are also many small blisters in the mouth, which give great pain; particularly those on the edges, and under the tongue. He cannot bear the light for an instant; and the attempt to open the eye-lids causes a copious flood of tears, by the acrimony of which the tarsi and cheeks are red and excoriated. The pulse, however, is only 85.

11th.—No fresh eruptions have appeared; many of the older vesications

vesications are dried, and covered with an incrustation; but others still discharge copiously, particularly those on the scalp, from which it is so profuse as to wet his night-cap and pillow-case in a very short time; the edges of the tongue are much ulcerated.

12th.—He feels very sore, and the excoriations are so general and extensive as to make every position irksome. He cannot allow his eyes to be inspected. The pulse is 100, and he is low; but takes some arrow-root and wine.

14th.—There were no new vesicles yesterday, but three have since appeared on the neck. The pulse is 80 in the morning, but considerably quicker in the evening.

15th.—No farther eruption; but the general irritation and soreness are very distressing, and the discharge continues copious, particularly about the joints, and from the parts upon which he presses; the eyelids are closed, ulcerated, and much swelled.

16th P. M.—The pulse rises in the evening to 110, and he feels very sore and feeble; but no fresh vesications have arisen. From this period that the eruption ceased, his health and strength declined; and the exacerbations in the evening increased. It was vainly attempted to support his strength by a more nourishing diet, wine, a decoction of bark, and by opiates to procure rest. Notwithstanding every attention paid to his unfortunate case by Dr Douglas, the surgeon of the hospital ship, he became every day more feeble and emaciated, and died on the 29th of January, perfectly exhausted and worn out by the sufferings caused by the irritation, soreness, and discharge. Although some marks of disease were discovered on dissection, and particularly in the thorax, yet the viscera in general were much sounder than they were usually found in those who had died of fever, or its consequences. The small intestines, however, contained a yellow, glairy, gelatinous-looking matter; and on their outer surface several yellow patches were observed; but it was difficult to determine whether these depended on an exudation of the same fluid between the coats, or upon the peritonæum having been elevated so as to have formed distinct vesicles at an earlier period of the disease. No vesicles were discovered on the inner surface of the intestines, although in some places it looked redder than natural. The contents of the right eye were found destroyed by an abscess, and the cornea of the left was opake and ulcerated; the arm-pits, groins, scrotum, and between the fingers, buttocks, &c. were perfectly raw, like a blistered part; and in other places the skin appeared dried and puckered up, and of a deep purple colour.

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Of the third case, likewise a Russian, I did not preserve any account, as fever prevailed to a great extent at the time; and it was divested of interest, by nearly all the vesicles having been ruptured before his admission into the Trusty, hospital-ship. They had been numerous, but were not succeeded by ulceration nor any constitutional symptoms worthy of notice; and he was well again in about a fortnight; the exudation from the vesicated parts generally forming a rough disagreeable brownish incrustation, which gradually peeled off.

The fourth case I did not see, as it occurred between the two periods of sickness, during which I was superintending physician of the Imperial Russian Fleet; but I have been favoured with some information respecting it by Mr Dobson, surgeon of the Trusty, hospital-ship, which I shall give nearly in his own words:

JACQUES JEJAURNOY, a French prisoner, ætatis 48, was admitted on the 25th of April 1813, labouring under cough, purulent expectoration, hectic sweats, and other symptoms of confirmed phthisis pulmonalis. In the night of the 5th of May, after a profuse perspiration, he was annoyed by a troublesome itching of his thighs; and on putting down his hand to scratch them, he felt several elevations of the cuticle, about the size of a large pea, but paid no farther attention to them at the time. The itching returned the next night, and more vesicles formed; and, on the following morning the first vesicle broke, as the patient asserted, with an audible noise. After this period some rose on his left side; and on the evening of the 9th one made its appearance on the left cheek. They continued to form and to burst in various places successively until the 16th of May; subsequently to which date no fresh blebs appeared, though some of the former did not break for several days after that period. The largest, which was upon the thigh, was, on the 14th, about the size of a large nutmeg; on the 15th it had become elongated, and measured an inch and three quarters in length, and about an inch in breadth. It did not appear to be altogether filled with serum; but, as it recovered its elevation, after pressure, seemed to contain a small portion of a gazeous fluid; a circumstance which, if my recollection be not erroneous, is noticed by some writer, and which is agreeable to the etymology of pemphigus.

The appearance of the eruption, as in the second case, seems to have been somewhat critical; for Mr Dobson mentions, that the man's health and appetite improved so much, and the phthisical symptoms were so far suspended for a time, that he felt a strong

strong inclination to inoculate some other consumptive patients with the fluid, but was deterred by some accounts of the formidable nature of this disease. A smarting sore followed the bursting of the vesications; but there was not any inflammation round their bases; nor did they leave any pits, as they generally became dry in two or three days, and terminated in a brownish-coloured desquamation.

A French assistant-surgeon, who was then in the ship, stated that he tasted the fluid, which was of an acid quality; and that it turned the red threads of the rug upon the bed of the patient, upon which it fell, to a dirty brown colour.

Remarks.—It has been considered extremely questionable, whether pemphigus ought to be regarded as an idiopathic, or a symptomatic affection. As in some instances recorded of fatal epidemics, accompanied with vesicular eruptions, these appear to have been merely symptomatic; and as, in better authenticated cases, such eruptions, as well as other exanthemata, have been occasionally observed in the more malignant forms of fever and of the plague, there is certainly ground for the doubts entertained by authors, and more lately by Dr Bateman, of the existence of pemphigus, as a peculiar eruptive fever.

The case last related will probably appear to have been a symptomatic affection, resembling the Pompholyx diutinus of Dr Willan, and excited, like the miliary eruption, by the profuse perspirations; but in the other patients no suspicion of such a cause, nor of a connection with any other complaint, could be discovered, and, therefore, I am inclined to view them as instances of primary disease.

The degree of attendant fever accorded perfectly with the description of the case seen by Dr Cullen, "Hominem leviter febricitantem;" for although the pulse and skin were generally somewhat affected, yet the febrile state was so mild as to cause but little, if any, derangement of the functions.

Neither, except in the case of the French prisoner, who complained of a very troublesome tingling and itching, could I satisfactorily ascertain if any peculiar sensation preceded or accompanied the eruption, as the inconvenience was so trifling, that the other patients were not aware of any thing being the matter with them until it appeared. This, however, might arise from the difference of language, and their greater insensibility and inattention to slight impressions, but their first, and indeed almost their only complaint, was of the smarting and soreness which followed the bursting of the vesicles.

The appearance and duration of these blains were so various, that they often existed in every stage of advancement at the same period.

period. They were of an irregular shape, and much resembled the elevation and effusion under the cuticle, produced by a partial burn or scald; and though most frequently the bulk of a filbert, they varied in this respect from the size of a pea to that of a large walnut, and were sometimes even larger.

Their duration, and the degree of subsequent discharge and excoriation, depended much upon the size and situation of the vesications; and, upon their being rubbed or pressed upon, where the skin was contiguous, or formed folds, the discharge from the ulcerated surface proved considerable, and the patient's sufferings distressing; and thus while smaller blisters, and those less exposed to irritation and pressure, soon dried up, and healed without difficulty, others were of much longer continuance, and left behind them marks of a deep red colour.

The partial inflammation, excited by the course of the effused serum, is very different from the general diffused redness of erysipelas, and therefore it is easily distinguished from the vesicular species of that disease, by the absence of surrounding erythema and tumefaction, and from other eruptive diseases, by the size and transparency of the vesicles.

In the first two patients, a severe degree of ophthalmy followed the rupture of the blisters on the eyelids, &c. and the excoriated parts generally were, in them, less disposed to heal, from the greater acrimony of the effused fluid, and a higher degree of constitutional irritability. The subjects were all, however, of spare, delicate, and irritable habits: the first, as already stated, said that he had been affected with the same complaint several years before; and one of their surgeons mentioned that it was not uncommon in Russia; but as he did not see the eruption in the first instance, I was not perfectly satisfied of his correctness as to the identity of the disease.

Notwithstanding the high authority upon which a distinction has been founded between pemphigus and pompholyx, the former being characterized by vesicles with inflamed bases, and attended by fever; the latter by vesicles without any inflammation round them, and without fever; yet, in so far as I may be permitted to judge from the above cases, I am inclined to consider pompholyx as a more appropriate appellation than pemphigus; and, as they appear to me to differ in degree only, to prefer their arrangement under one generic character, as in the valuable work of Dr Young.

That the distinction in question has originated from the descriptions given of those vesicular eruptions, which are occasionally symptomatic of pestilential fevers, seems to me highly probable from the following extract from the directions given to

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the searchers, by the College of Physicians, during the plague in London, in 1665, to take notice "Whether there be any blains which may rise in any part of the body, in the form of a blister, much bigger than the small-pox, of a straw colour, or livid colour, which latter is the worser; either of them hath a reddish circuit, something swollen about it, which circuit remains after the blister is broken, encompassing the sore."

This, I think, clearly explains the inferred malignancy, and the fatal accounts given of pemphigus by some writers; for in such instances, as in the plague, those eruptions appear to have been purely symptomatic, and are evidently characteristic of a highly malignant disease.

With regard to the constitutional excitement, often vaguely termed fever, which, even in the plague itself, is occasionally absent, it can hardly, with propriety, be deemed pathognomonic in pemphigus, since in no respect do diseases vary so much in themselves as in the nature and degree of the accompanying pyrexia; and however useful the multiplication of generic characters may be for the purposes of arrangement, yet we are forced to acknowledge that, in practice, we find gradations and anomalies in disease, everywhere encroaching upon the limits assigned by the nosologist.

The nearly simultaneous appearance of the two cases in the Argonaut, might, at first sight, suggest the probability of the disease having been communicated by the one to the other; but there appeared no ground for this suspicion; for though, in both ships, crowded with patients, there was every facility of intercourse, yet the disease was not propagated in a single instance. Moreover, with a view to determine if it possessed any contagious power, inoculation, from the vesicles of the case that afterwards terminated fatally, was practised, on two occasions, without producing any effect. Inoculation was also tried in erysipelas, with a similar result.

Upon the whole, pemphigus differs in some essential points from the character of the order Exanthemata; which, indeed, is only applicable to the contagious eruptive fevers.

It appears to depend upon a peculiar state of the constitution, influenced probably by diet, mode of life, or local situation, and to be characterized by the successive eruption of pellucid vesicles, from the size of a pea to that of a walnut, which, usually breaking in a few days, discharge a yellowish serum, and are generally attended by a mild degree of fever.

XII.

Observations on the Use of Ipecacuanha and Opium in Dysentery. By Mr William English, Surgeon.

In your Journal for January 1813, Mr Playfair has obliged us with his ingenious method of curing dysentery in Calcutta, by the means of large doses of ipecacuanha and opium. I am happy to bear witness to the efficacy of the remedy given in the way he proposes in this country; and it is chiefly in consequence of this quotation from his excellent paper, viz. "It must, however, be remarked, that this treatment was always adopted at the commencement of the attack, for when the disease has been of any standing, the stomach is too irritable to retain the medicine for an instant," that I am induced to lay before you the following case, which may tend to prove, that the beneficial effects of this medicine are not confined to the early stage of dysenteric complaints, and that equally happy results may be hoped for at a later period.

John Lock, aged 28, seaman, has been about four years on board his Majesty's ship President, on the Cape of Good Hope station: was seized with the flux, 28th February 1813, on the passage from Saint Helena to England, which continued unabated on the ship's arrival, when he was sent to the Argonaut hospital-ship, Chatham, on the 21st May, for, as his certificate says, "cure of chronic hepatitis and obstructed abdominal viscera," and was discharged uncured, 28th August, to go to his place of residence, being found unserviceable and invalided. About the 17th September, he was admitted into Guy's Hospital, and remained there three weeks, when he was discharged, at his own request, much in the same state as he went in. He was afterwards under the care of a very respectable physician, and likewise of an apothecary of this city, for about a month. On the 8th November 1813, I was requested to see him. He gave me the above history, showed me his certificates, &c., and said his complaint was still the same as it had always been, notwithstanding he had been taking medicines for it ever since he was first attacked. His appearance was misery and starvation itself; he had a most voracious appetite for food, and insatiable thirst; which, as soon as gratified, produced irresistible inclination for stool, with distressing tenesmus and much griping pain, the evacuations

cuations being chiefly blood and mucus; and as he was constantly either eating or drinking, so likewise were the motions almost incessant. He shewed me fourteen stools he had passed during the night; the quantity was great, and it is surprising how he could bear the loss of so much blood, which he positively declared was not more than he had nightly discharged since his illness commenced. In the day the purging was more frequent than in the night, but I thought there was less coagulated blood in what I saw in the evening; pulse 120; skin hot and dry. At bed-time he took fifty grains of ipecacuanha, and sixty drops tinct. opii in an ounce of cinnamon water, soon after which he fell asleep, and slept for three hours. He then awoke and vomited twice, and had a stool without pain, consisting of dark-coloured, grumous, coffee-ground like substance, and slime, with some feces; and about an hour after he got into a sound sleep, and so continued until morning, when he arose and had another stool without blood, and but little slime. This evacuation was partly figured, the first he had seen since his illness; he felt himself wonderfully relieved, his thirst was moderate, and his appetite much less craving. During the 9th November, he only had three stools, without pain or blood. At bed-time he took the draught again, which, being vomited in a short time, was repeated. This he retained, and slept until breakfast-time in the morning. He had one stool in the night, after vomiting, and another in the morning,—a copious natural-coloured figured stool without pain. During this day he scarcely complained of thirst, and his appetite, though good, was easily satisfied with the usual meals. I was now taken seriously indisposed myself, and did not again visit my patient, but he called on me a few times, and I ordered him three draughts, with a scruple of ipecacuanha, and sixty drops of the tinct. opii in each, to be taken every night, and requested him to leave the close confined lodgings he had in a narrow ally in Tower Street, and go to his friends in the country, considering the disease as subdued, though the effects were by no means overcome. This advice, I understand, he did not comply with, for about a fortnight, until he found he rather lost than gained strength, and his purging threatened to return, when he commenced a journey to Dorsetshire, where he soon got quite well; since which he has resumed his usual occupation of a sailor, and continues in good health.

I will take this opportunity to impart, that when serving in the East India Company's employ, I observed, that not only in dysentery, but in almost all febrile attacks, the stomach was excessively irritable; and when an emetic was given in the common way, it was instantly rejected, and seldom produced any beneficial effect; but on the contrary, it afforded a pretence for swal-

lowing

lowing large quantities of chamomile tea, conjee, &c., with an idea of working it off, which invariably aggravated the complaint, by disordering the stomach still more. From this observation, and remarking the sickness was not attended with that peculiar deadly faintness attending the proper action of a dose of ipecacuanha, from whence perhaps its greatest efficacy is derived, I was induced to stay its operation by combining opium with it, when I wished to have the full advantage expected from the power of an emetic; and I am quite convinced, that, after adopting this plan, particularly in dysentery, where an *emetic was*, in my opinion, especially indicated on its commencement, I succeed infinitely better in the recovery of my patients; indeed, Mr Playfair and I seem to have had the same results from the same remedy, although given with a very different view. My idea was purely to produce the effect of an emetic on the system, not merely ejecting the contents of the stomach, but to excite that peculiar action of the whole frame, better understood than expressed, by which I consider the undue irritation of the bowels, and the consequent secretion, were appeased. I gave half a drachm of ipecacuanha, with thirty or forty drops tinct. opii, and if this did not excite vomiting in an hour, as it sometimes did not, it was repeated, with less opium in the second dose.

It is unnecessary to add, that the dose was always repeated, when it was ejected as soon as swallowed; and sometimes it was necessary to combine it with an effervescent draught, given in as little bulk as possible. Perhaps for some of my younger brethren (those who visit warm climates), it may be worth while to remark, that ipecacuanha and opium, as above, allay the spasmodic irritation of the stomach, when opium combined in any other way has little or no soothing effect; the same as a large dose of rhubarb and opium is observed to affect the bowels. Half a drachm of rhubarb, and 60 or 80 drops tinct. opii, will often give a respite to the most incessant purging, when the same quantity of opium, either alone or with astringents, &c. has no such effect

Mark-Lane, London, August 10, 1814.

XIII.

Observations on the necessity of Opening the Sac in the Operation for Hernia. By J. Penkivil, Surgeon, Plymouth Dock.

Some remarks, by an anonymous writer in your last number, having attracted my attention, which appear calculated to do much

much mischief, if adopted in practice, I am induced to trouble you with a few lines. The compliment paid to the different modern writers on the subject of hernia, is such as, I believe, the whole profession subscribes to. I recollect with pleasure and gratitude, the excellent and invaluable lectures of the venerable professor of Edinburgh, Dr Monro, sen.; but, with all due respect for such high authority on this subject, I remember not to have been so thoroughly satisfied; and subsequent reference to the authors who have written so ably on it, such as Hey, Lawrence, Cooper, &c. as well as my own experience, convinces me of the impropriety of returning, or attempting to return, the sac unopened, in the operation for strangulated hernia. The whole weight of the argument for returning the hernial sac unopened, seems to rest on the dread of the stimulus of the atmospheric air. Undoubtedly every stimulus should be avoided on an occasion when the parts concerned are already too highly stimulated, and where too great action previously exists. But is the atmospheric air the only enemy we have to contend with? Surely other circumstances necessarily connected with the operation are overlooked, and, if we surmount them, I think it will be difficult to prove we ever lose a patient from the mere stimulus of atmospheric air. It is possible this may prove an aggravating stimulus, combined with other causes; but this alone, I think, is not to be considered the occasion of our failure in this operation. It seems, according to the reasoning of your anonymous correspondent, that it is on opening the sac the peritoneal inflammation takes place. Yet is not the external layer of the peritonæum exposed to the action of the air when the sac is not opened, and is it not equally unaccustomed, and equally liable to be stimulated by it, as the internal layer is, when the sac is opened? The inflammation thus excited will be as readily communicated and extended in the one case as the other. If so, the peritoneal inflammation, so much dreaded, is as likely to occur one way as the other. If the operation for strangulated hernia could be reduced to the simple incision and easy return of the unopened sac, as described by your correspondent, the motto "*utrum horum mavis, accipe,*" would be quite unnecessary. But it appears to me something more is necessary. It is admitted by the same writer, that cases occur in which it is necessary to open the sac. If so, what are the particular circumstances which will direct us what line of practice to adopt? We are left here to a blind chance; for there are no symptoms which will infallibly serve to distinguish what may be the contents of the sac, or what the state of those contents, if attempted to be returned unopened. Again, supposing that, in every case, we should return the

sac,

sac, we are not sure of relieving the stricture, the cause of strangulation; for if the stricture be at the mouth of the sac, we may return it within the abdomen, fancy all is well, and yet lose our patient. A case of this kind I remember to have witnessed a year or two ago, under the care of a medical friend, who did me the honour to consult me. The hernia was reduced by the taxis, but the symptoms of strangulation were not relieved, and the patient died. The body was not permitted to be opened; but I have no doubt the stricture was at the mouth of the sac, and, though the hernia was reduced, the stricture not being relieved, the return was of no avail. There are, however, cases where it is impossible the sac can be returned. Adhesions are often formed between the sac and surrounding parts, which cannot be successfully separated. What is to be done in strangulated herniæ congenitæ, where the history of the case is imperfect, and where the nature of parts is so altered, either from there being an accumulation of fluid, or an effusion through the surrounding cellular membrane, as to make it difficult, if not impossible, to ascertain its kind? A case of this sort occurred in my own practice a few weeks ago, in which I was obliged to operate. The patient had given an inaccurate history of his case, which first deceived me and my professional friends who kindly visited with me. There was such a general effusion throughout the cellular membrane of the scrotum, that, nothwithstanding our diligent and patient examination of the parts, not one of us had an idea that it was congenital hernia till I had laid open the sac, and returned the intestine, when the testis most unexpectedly was exposed to view, proving it to be congenital hernia. Suppose here I had attempted, on laying bare the sac, to return it unopened, what embarrassment to myself and danger to the patient must have ensued? But I am happy to say, notwithstanding the opening the sac, the exposure of a considerable part of ileum, and a small portion of the colon to the *atmospheric air*, and the additional cause of irritation, from the laying bare the testis, my patient is recovered, and walks about as well as ever. Another objection to returning the hernial sac unopened, even if it were in every case practicable, is, that in so doing you know not what you return, or in what state the contents may be, whether mortified or not. Surely it is of consequence to ascertain this, which can be only known by opening the sac. Your correspondent admits, that, beyond doubt, the sac must be opened when mortification has taken place, in order to remove the gangrenous portion. But, supposing this necessary, are there any infallible rules by which we can ascertain exactly when mortification has actually commenced? If

If we are guided by symptoms alone, however accurately described, and however narrowly watched, I fear we shall err. Nothing but actual examination, *i. e.* opening the sac, can decisively ascertain it.

If I understand rightly the gentleman I am now answering, he seems to recommend, in the treatment of strangulated hernia, a cathartic enema, to the exclusion of one of tobacco. Now, the only possible good a cathartic enema can do, which it may be right to try, is to remove feculent matter deposited below the stricture, whereas an enema of tobacco produces such general relaxation, that it prevents, most frequently, the necessity of the operation, of which I have seen repeated instances. Indeed, this is now so generally known and adopted, that it seems our last resource previous to an operation. Whilst I am a decided advocate for an early operation, where the usual means of the taxis, bleeding, warm-bath, and tobacco enema have failed, yet your correspondent, I think, is rather hasty in confining us to one hour. Much must depend upon the symptoms being urgent, or otherwise. All the remedies mentioned have been tried; an opiate with antim. tart. have been given at night, and the following morning the taxis has succeeded with the greatest ease. Certainly, after the usual means have been tried in vain, every hour the operation is delayed, we plunge our patient in much greater danger. But still every case has its peculiarity, and by its symptoms we ought to be guided.

By way of recapitulation, the arguments then for opening the hernial sac are, That in very many instances it is utterly impossible to return it unopened; to these might be added cases of strangulated congenital hernia, and which cannot always be previously ascertained: That, were it in all cases possible, it would be improper and unsafe to do so, since we could not know what might be the contents, or, what is more essential to the welfare of the patient, we could not learn the state of those contents, whether fit to be returned or not: And, lastly, That where the stricture may happen to be at the mouth of the sac, the protruded sac, with its contents, might indeed be returned to the cavity of the abdomen, but the stricture not being relieved, the patient must inevitably perish. Now, against these weighty reasons, we have only to balance the flimsy theory about atmospheric air, and the rough treatment the patient may be supposed to undergo from the rude hand of an uncouth surgeon!

The other remarks of your anonymous correspondent, I think, are so obvious, that no one is likely to dissent from them.

Plymouth Dock, August 17, 1814.

PART

PART II.

CRITICAL ANALYSIS.

I.

Facts and Observations relative to the Fever commonly called Puerperal. By JOHN ARMSTRONG, M. D. &c. &c. 8vo. pp. 162. London, 1814.

WOMEN, after delivery, are subject to several acute diseases, all of which may be, and indeed have been called puerperal. In the greater number of these, the practice is fixed, and upon the whole successful. But the name of puerperal fever has been more especially assigned to one infinitely more insidious and more dangerous than all the others; to one, which has at different times appeared with the character of a low malignant epidemic, in the lying-in hospitals of London, Dublin, Paris, and Edinburgh, and in the private houses of the lower order of child-bed women. This puerperal, or low child-bed fever, has generally proved highly unmanageable, and extremely mortal. All the women who were attacked with it in Dr Young's wards at Edinburgh, perished; the greater number of those cases which occurred in the Hotel Dieu, proved fatal; and Dr John Clarke has observed, that about three fourths of all the women who are attacked with the low fever of child-bed, die. In the treatment of this disease, physicians are so far from being agreed, that we find blood-letting, and other antiphlogistic remedies, recommended by some,—and bark, opium, and wine, by other equally eminent and experienced practitioners.

We suspect, however, that the features of puerperal fever, strictly so called, are far from being well defined; and that, under this title, very great varieties of disease have been described and treated by different physicians. The diagnosis between puerperal or low child-bed fever, and the more common forms of peritoneal and uterine inflammation which occur in the puerperal state, is, in our opinion, still obscure and undetermined. We shall even venture to assert, that there is an almost imperceptible [illegible]

dation or series of cases to be observed in actual practice, from the most clear and intelligible case of ordinary puerperal peritonitis, to the case of low child-bed or puerperal fever. The extremes of these are distinguished, in the first case, by frequent hard pulse, intense heat, and thirst, acute pain of the belly, tension, fulness, and other undoubted symptoms of local inflammation; and, in the last case, by a pulse more frequent, more full and soft, and by obscurer signs of local inflammation. No man can mistake the nature of the first; but in the last, the patient complains more of the head than of the belly. In some few cases, she perseveres, even after much cross-questioning, in denying the existence of pain in the belly; and then the physician, though he may very early discover a degree of roundness and fulness, will, at the same time, find the abdomen wants the feel of tension that it has in other cases. There is reason, too, to believe, that other cases of acute febrile affection, occurring in the child-bed state, such as the ephemera, or weed, suppression of the lochia, and inflammation of the uterus and its appendages, have been mistaken for puerperal fever. This is alleged to have been the case at the Hotel Dieu, at the time of the experiments which established the reputation of Doulcet and his powders. To every lying-in woman who fevered in the Hotel Dieu, the ipecacuanha was given; and it is now deemed no way extraordinary that the greater number of those women recovered.

Something of this kind, we are inclined to think, has taken place in other instances, on the occurrence of alarm from puerperal fever. One or more deaths occur in women who have fevered after child-birth; practitioners are alarmed, and become observant, and the ordinary cases of pyrexiæ are noted as recoveries from an epidemic puerperal fever. We really, however, can hardly conceive, how any of the diseases incident to lying-in women can be mistaken for the true puerperal, or low fever of child-bed, unless it be the case of puerperal peritonitis; and the limits bounding some of these cases are so indistinctly marked, that the one case, as we have already remarked, seems to pass imperceptibly into the other.

This is the declared opinion of the author of the well-written essay now before us, who includes under the term puerperal fever, both the ordinary peritoneal inflammation, and the low malignant fever of lying-in women. This conclusion seems necessarily to have arisen out of his observation of the course of the puerperal epidemic, as it appeared in Sunderland and vicinity, in 1813; and the history of which Dr Armstrong has here undertaken to record. "The few cases which took place in the

winter were simply inflammatory; without exception, they yielded to an antiphlogistic practice, and the majority of them to brisk purgation, and a spare regimen. In the spring of the year, however, the disease became more frequent, and much more formidable, being attended with symptoms resembling those of a malignant typhus; and, in little more than two weeks, five fell a victim to its violence."

In the first section of this essay, the symptoms of the disease are very fully and methodically narrated. The description professes to be a copy of the appearances assumed by the disease at Sunderland and the adjacent neighbourhood. The symptoms are properly classed under two stages; a classification of great importance, as it is only in the former stage that active measures can insure safety to the patient. The first stage may be considered as the one of inflammation or excitement; the second, as that of effusion, and marked by the symptoms of debility, malignity, and approaching dissolution.

The first stage seldom continued longer than fifty hours, and in some it terminated much sooner.

"When the disease was not impeded at this period, it passed into what may be termed the second and last stage; which, toward the close, was marked by an exceedingly great prostration of all the vital powers.

"But the first approaches of this fatal stage were most clearly indicated by the rising of the pulse, which then generally ranged from between 140 and 160 in the minute, and was very soft and compressible; it feebly struck the sides of the artery, and gave the idea that the heart was labouring hard to keep up the force of the circulation. About twelve hours before death, the pulse became thready, fluttering, and irregular, and so rapid as not to be correctly numbered.

"For some time after the accession of the second stage, the skin remained at an increased temperature and dry, but then the patients almost constantly complained of chilliness. The cheeks were alternately flushed and deadly pale, the eyes lost their lustre, the pupils were much dilated, and a kind of dewy perspiration stood upon the face and forehead. The pain gradually and entirely receded from the surface of the abdomen, when it usually happened that dark, slimy, and very fetid stools were discharged from that time onward. The thirst was unceasing, and when any liquid was offered, the patients hastily seized the vessel, and glutted down its contents, as if they had previously been expiring for want of drink. The tongue for the most part was brown, or rather black and parched, and had aphthæ upon it, which even appeared about the edges of it at an early period. In one very bad case, however, the tongue continued clean and moist to the last, but there was an almost perpetual vomiting throughout the second stage, though only a slight nausea occurred in the beginning, and very little vomiting in the rest of the first stage. Indeed

Indeed vomiting was always more urgent in the last than in the first stage of the disease, and the matter then thrown up very much resembled coffee-grounds, and was offensive to the smell. The teeth and gums were crusted with dark, slimy sordes, and the breath was disagreeable, as if it had been tainted with mercury. Throughout the complaint there was a short teasing cough, but this was more especially the case in the last stage, when the respiration grew very short, feeble, and frequent, and the alæ nasi were thrown into perpetual motion.

"Soon after the advancement of the second stage, the patients began to talk incoherently, they frequently made attempts to get out of bed, and occasionally, after having lain still a short time, suddenly started, and spread out their hands, which were then very tremulous, as if to ward something off that was approaching them. About this time, two patients became gradually collected, complained of no pain whatever, looked and spoke cheerfully, and flattered themselves that they would soon be well; this illusion continued till within an hour or two of their departure, rendering them completely insensible to their real situation; and even to friends, though warned by the medical attendant, their death was at last unexpectedly sudden. But in three other unfavourable cases, the light wanderings of the mind which took place at an early period of the second stage, were not succeeded by a state of serenity, but by a low muttering delirium, speedily followed by a stupor, in which the patients lay with their eyes half-closed, and could not be roused from it but by loud speaking, upon which they started as from a disturbed sleep, uttered some vague and hasty expressions, and then sunk into the same condition as before. A few hours before death, in these cases, some dark scattered petechiæ appeared, and the skin was in that peculiar state which accompanies the last stages of tetanus, and the nervous fever of intoxication; the whole surface felt soft, relaxed, and clammy, and the hand glided almost as smoothly over it as if wet by soap and water. In the above three instances, also, slight stertorous breathing occurred near the termination of the disease, and, last of all, general though not violent convulsions."

This seems a very faithful and picturesque representation of the horrors of child-bed fever. It is the description of the second stage, from which he acknowledges none recover; and, therefore, we may be well assured, that if Dr Armstrong had often witnessed such a train of symptoms, the fever which he has described must have been fully as mortal as heretofore,—that is, as Dr Clarke has it, three-fourths of all who were attacked must have perished. But Dr Armstrong had the good fortune to witness only four deaths from this disease: and therefore we may infer, that he saw this horrid train of symptoms no oftener; and indeed, during the whole reign of the epidemic, only

ly five deaths occurred in the collective practice of Sunderland and vicinity.

"From accurate documents now before me, it appears that, from the first of January to the first of October, 1813, forty-three distinctly marked cases of puerperal fever have occurred to five practitioners residing in Sunderland, and the adjacent parishes of Bishopwearmouth and Monkwearmouth; four of these gentlemen I met in consultation on different occasions, and I can assert upon the testimony of them all, united to my own, that only five cases out of the whole number terminated fatally. By comparing these facts with the reports as to the general fatality of puerperal fever, and by examining the evidences contained in the appendix, the superior advantages of the practice pursued will be fully apparent.

"The thirty-eight successful cases were all treated by copious depletions of one kind or other, and in twenty-nine of them, calomel was exhibited in doses of a scruple, or half a drachm at the beginning, and occasionally repeated in the course of the distemper. For the most part, it passed so expeditiously along the intestinal canal, that there were very few instances in which ptyalism was excited, and whenever this was the case, it seemed a favourable circumstance, all the patients, with only one exception, recovering with more than ordinary celerity from the time that the mouth became affected. And further to illustrate the superior efficacy of large doses of calomel, it may be here remarked, that in none of the five cases which proved fatal, more than fourteen grains of calomel were given on the accession of the fever, jalap, sulphate of magnesia, and castor oil, being the cathartics chiefly employed during its progress."

This extraordinary success we would willingly attribute to the active and well-timed practice of Dr Armstrong and his brethren of Sunderland; though we are aware that some may be disposed to suspect, that the greater number of the cases were not examples of the low child-bed, malignant puerperal, fever. It must be recollected, that Dr Armstrong acknowledges the disease to have been in the beginning more inflammatory, and that he considered the ordinary cases of puerperal peritonitis to differ not in kind, but in degree only, from the low puerperal fever. In both, he considers the inflammation of the peritoneal covering as the essential part of the disease; and his object is to show, that the same depletory and antiphlogistic method is appplicable to both; that, indeed, from the more rapid course, and more fatal tendency of the low puerperal fever, these powerful remedies become the more indispensable; and that it is only by an early and vigorous use of blood-letting and purging in the first stage, that the disease can be cut short before passing into the second and destructive stage, from which few or none can be recovered by

by any method. In this stage, indeed, blood-letting could only hasten the fate of the patient. It is in the very beginning of the disease that this remedy can be employed; and, perhaps, we shall not err in saying, that, in puerperal fever, it is in the first twenty-four hours only that it is likely to prove useful,—so soon does the stage of excitement pass away. This seems to have been the opinion and practice of that judicious physician, and experienced accoucheur, Dr Denman, who has laid it down somewhat in terms of a rule, "that it is in general absolutely necessary to bleed in the beginning of puerperal fever." But he remarks also, "If the first stage be passed over, bleeding will certainly then be injurious, the opportunity having been lost; and the physician afterwards called in, however great his talents may be, will too often have the mortification of being a spectator of mischief, which he cannot then remedy, and of an event which he can only deplore."

The successful treatment of puerperal peritonitis and fever, depends so much upon an early application of energetic remedies, founded on a just apprehension of the two stages of disease, that we shall here quote Dr Armstrong's summary observations on this subject.

"In the first stage, after the rigors have ceased, the pulse is hardly ever less than 120, and sometimes, though as far as I have observed very seldom, as high as 140 in the minute; the blood does not seem to flow in a soft, easy, natural current, but comes against the finger with a kind of vibratory motion, and more than ordinary pressure is commonly requisite to stop its course along the artery, which feels rather hard and tense. The skin is dry and hotter than natural, the patient complains of great pain and soreness of the abdomen, breathes nearly forty times in the minute, vomits mucus and bile, is generally bound in the belly, has a white, dry tongue, considerable thirst, and labours under all the restlessness and irritation of fever.

"In the second stage, the pulse is never under 140. and frequently rises above 160 in the minute, while it is always exceedingly variable, weak, and compressible; the tenderness of the belly is usually much diminished, and the fulness increased; cold partial perspirations first break out about the face, neck, and extremities, the centre of the body, particularly the surface of the abdomen, remaining dry, and of a pungent heat for some time afterwards; the patient rarely shivers much, but has repeated chills, vomits dark grumous matter, seldom breathes less than 60 times in the minute, has generally a loose belly, a brown, black, or reddish parched tongue, unquenchable thirst, tremulous hands, lightness and swimming of the head, confusion of thought, or delirium, and, several hours before death, a remarkably relaxed, cold, damp skin. The first stage is marked by highly inflammatory, the second by highly typhoid characters; and it has always

ways appeared to me, that the tendency to putridity in the latter was proportionate to the degrees of inflammation in the former.

Perhaps scarcely any of the above symptoms, taken singly, can be entirely depended upon as distinctions between the first and second stage, yet, as several of them accompany or succeed each other, in each of these states of puerperal fever, they may together enable the observant practitioner to form a tolerably correct opinion relative to the plan of treatment to be laid down. In the first stage, blood-letting is generally followed by the most beneficial, in the second, by the most fatal effects; an antiphlogistic regimen is indicated in the first, but the second requires a nutritious diet.

"The first stage is variable as to its duration, sometimes terminating in little more than twenty, and sometimes continuing as long as seventy hours, but always being much shorter in the epidemical than in the peritonitic fever. The period of time which the second stage occupies is likewise very uncertain; if it be accompanied by gangrene it does not last many hours, if by suppuration, it is generally mortal in three or four days, and if by an effusion of serous fluid and coagulable lymph, without an actual disorganization of a vital part, it may continue a longer time, and perhaps present the possibility of recovery, but seldom any thing more; for very few patients are saved in the second stage, whatever may be the morbid changes with which it is connected."

No practitioner, we think, will deny that these observations are descriptive of puerperal fever. The other claims of the Sunderland epidemic to that character, are, the frequent occurrence of the fever amongst child-bed women in Sunderland and its vicinity, and even in other parts of Durham and Northumberland at that time; its appearing to be propagated by contagion, from the circumstance of its occurring more especially amongst the patients of particular accoucheurs; and, lastly, from the low and typhoid character which it assumed in some cases.

The remedies, according to Dr Armstrong, on which our chief dependence can be placed in the treatment of this disease, and which have proved so eminently successful in the Sunderland epidemic, are blood-letting and purging.

With regard to the practice of purging freely in every variety of this fever, there can now, we apprehend, be little difference of opinion. If there is any one point of practice, in which all who have written on puerperal fever agree, however much they may differ on other points, it is in this,—the propriety and utility of early and repeated evacuations by purgatives. And though Dr Armstrong earnestly argues the propriety and advantage of blood-letting in the very earliest attack of the disease, it is clear, even from his own account, that purgatives are more generally applicable,—that they are indispensable in every case, whether

whether venesection may have been practised or not,—that, indeed, many cases were recovered by active purging alone,—but not one by venesection, unaided by purging.

"Although strenuously insisting upon the utility of phlebotomy at an early period of the first stage, yet it was never my intention to affirm, that it is of itself equal to the removal of puerperal fever; on the contrary, I feel it my *duty* to declare, *that it has generally failed in my hands*, unless followed by *powerful cathartics*."

Now we understand Dr Armstrong to be speaking of his experience in *this epidemic*; and we are led to pause at this observation, and to ask the following questions, which we trust will at least be excused, on the score of the high importance of the subject. What cases were treated by blood-letting alone, without the aid of cathartics? what is meant by failure in these cases? It cannot be the death of the patients; for, during the whole rage of the epidemic, there were but five deaths. Four only of these were seen by Dr Armstrong, and it was not till after losing three, at least, of those four, that he, in concert with Mr Gregson, appears to have had recourse to the lancet. Or does it only mean, that, notwithstanding blood-letting had been practised, relief was not obtained till after the purgatives had been freely and successfully administered? We do not doubt that there are cases where previous blood-letting may be useful, and Dr Armstrong found it so; but it does appear to us, from a careful and critical perusal of the volume now under our review, that the very successful treatment of the Sunderland epidemic, is chiefly to be ascribed to the early, free, and repeated administration of powerful cathartic remedies.

The cathartics employed by Dr Armstrong were powerful ones. Calomel appears generally to have been given in doses unusually large, from a scruple to half a drachm at once, and followed by full doses of sulphate of magnesia, senna, or castor-oil; and the relief obtained in any case seems to have been proportionate to the free operation of these medicines, and the consequent evacuations of the black fetid stools, so characteristic of the disease.

In addition to bleeding and purging, Mr Gregson, surgeon in Sunderland, prescribed antimonial emetics, which appeared to be useful auxiliaries, though never used till the patient had been freely bled and purged. Another of Dr Armstrong's correspondents, Mr Wolfe of Chester-le-street, has always trusted to copious purging, unassisted by bleeding, for the cure of puerperal fever, and with great success.

The practice of Mr Gregson and of Mr Wolfe is given at length

length in their own words, in an appendix, in which also the reader will find particular cases of the Sunderland epidemic, illustrative of the disease, and the successful plan of treatment pursued by Dr Armstrong.

Upon the whole, this essay is well written, and well arranged. It contains much important observation, and many judicious remarks; and though we think there is still something wanting to complete our knowledge of the pathology of puerperal fever, we give to Dr Armstong the praise of having disincumbered the subject from much of its rubbish. We regret that he has not been enabled, by opportunities of dissection, to elucidate the morbid appearances presented by the parts affected by the disease. This is still a very important subject of inquiry; for though we have dissections on record, the reports of these are contradictory, and too general to satisfy the doubts of the zealous and accurate pathologist. As we are persuaded, from the ardour and intelligence of Dr Armstrong, that he will not drop this inquiry, we shall conclude by urging his attention to this important point. There is something very different in the appearances presented after death from puerperal fever, and after that from peritonitis or enteritis, occurring in cases not puerperal; in cases from cold or general causes; or in cases after the surgical operations for hernia, or calculus vesicæ.

II.

A Practical account of the Fever commonly called the Bilious Remittent, as it appeared in the Ships and Hospitals of the Mediterranean Fleet; with Cases and Dissections. To which are added, Facts and Observations, illustrative of the causes, symptoms, and treatment of Fever in the Mediterranean; comprehending the History of Fever in the Fleet during the years 1810, 1811, 1812, 1813; *and of the Gibraltar and Carthagena Fevers.* By William Burnett, M. D. Physician of the Fleet; late Physician and Inspector of Hospitals to his Majesty's Fleet in that Sea. 8vo, pp. 296. Callow, London, 1814.

The dominion of prejudice in medical practice is not to be overthrown by assault, or to be annihilated by a few attacks; but

but it may ultimately be undermined by a series of labours, and by the efforts of many individuals, until it shall crumble down by its own weight. Almost every new production of a practical nature, relative to the history and treatment of fever, tends to accelerate this desirable event. The accumulating experience, which a long continued war in various quarters of the globe has necessarily produced, by multiplying the occasions of disease as well as the observers, seems to be uniformly converging to the same point. It seems to be uniformly concurring in the rejection of the pernicious doctrines of putrefaction and debility as the essence of fever, and in demonstrating the ill success of the antiseptic and tonic or stimulant treatment, which originated in those doctrines; and of course in shewing at the same time the superior efficacy of early evacuations, and an antiphlogistic plan, in the commencement of all acute fevers. On every occasion that offered itself, we have endeavoured to contribute to the diffusion of this view of the subject among the profession, being convinced from considerable opportunities of observation of its truth, and of the importance of exploding altogether the opposite system. We are much gratified, therefore, in directing the attention of our readers to a great mass of evidence upon this topic, which Dr Burnett has brought before the public, much of it being obtained from his own immediate observation, and a considerable portion collected from others by virtue of his official situations.

Dr Burnett appears to have been employed in the medical department of the navy in the Mediterranean from the year 1799 to the present time; first as surgeon to a ship of war, and subsequently in the capacities mentioned in his title-page; a circumstance which at least evinces the extent of his opportunities of observation. The work is divided into two parts, the first of which contains a history of the summer and autumnal fever in those climates, with a general view of the plan of treatment, and of the effects of particular remedies, and an account of the appearances on dissection; which are followed by a considerable detail of cases and dissections recorded by the author himself, and several collected from other practitioners. The second part consists of a history of the fever in the fleet, *i. e.* in various individual ships, on different stations, and in different years;—of an account of the fevers of Gibraltar and Carthagena in 1804, and some subsequent years;—and of some general observations, with a short appendix.

Dr Burnett does not enter much into the controversy respecting the origin of these fevers; nevertheless, he states several facts, which evince the influence of miasmata in producing them. In the very first paragraph, after mentioning the general exemption of

of the fleet from these fevers during the spring and early part of summer; he says,

"And. with one exception, I am not acquainted with any instance where the crew of a ship has been attacked, after being some time at sea; the disease, in its epidemic state, being confined generally to such ships as are, or have been, refitting in port." p. 3.

Again, "The harbour of Mahon is divided by an island, on which stands the naval hospital: ships anchored during the summer or autumn *above* the Hospital island for any length of time rarely escape without a number of their men being attacked with fever; while the ships anchored below are commonly entirely free from it." p. 14.

This he exemplifies in two frigates, which lay three weeks in the beginning of October 1812 to refit, and sailed together off Toulon: the ship which had lain above the island had thirty-two men attacked with the fever; the one which anchored below not a single man. But it is unnecessary to multiply proofs of this kind. Where the predisposition had been thus given, it was uniformly observed that intemperance, exposure to the sun, or to night dews after a fatiguing day, were the most powerful exciting causes; and when the fever followed intemperance it was the most severe and untractable. Dr Burnett asserts that he was never able to detect the agency of contagion in the production of this fever, nor had he any reason to believe in its existence from the report of others; though he admits the probability of the conjecture, that, in the latter stages, where ventilation, and cleanliness were neglected, and the sick crowded, the surrounding atmosphere might be vitiated.

In offering our objections to the able and to a certain extent true doctrine of Dr Clutterbuck, in an early number of this Journal, respecting the exclusive seat of fever being found in the brain, the common occurrence of local congestion in other organs was much dwelt upon. The following observations of Dr Burnett seem to accord with that view.

"The train of symptoms, which have been just enumerated, will not always be presented in the same patient, nor is death itself uniformly preceded by such appearances as I have described; particularly when the disease terminates fatally before the third day. In such cases the brain is the part more immediately affected, and death has sometimes taken place suddenly; the pulse remaining full, soft, and even somewhat strong, till nearly the last moments.

"I have before observed, that, during the early part of summer, the brain is the organ most violently attacked; when the heat increases, and the periodical rains fall, the gastric symptoms become severe, but without diminution of the local affection of the head. As autumn advances, there is often an inflammatory affection of the intestines,

testines, frequently from improper treatment terminating in dysentery. In the winter months, this disease is often accompanied by severe and evident inflammation of the lungs. In the summer and autumn, slighter affections of the lungs are occasionally observed, but the patient seldom complains of this unless when asked." p. 11.

We may learn from the following paragraph how general the claim to the title of *yellow fever* is even in these Mediterranean fevers.

"There are few severe cases when the disease is protracted beyond the third day, and in which the gastric symptoms are urgent, that the yellow suffusion does not make its appearance in; and the earlier it is observed, and the deeper hue it assumes, so in proportion is commonly the danger of the patient; not only as to his present recovery, but also as to the ultimate consequence of the fever; as in almost every instance it portends a protracted convalescence," &c. p. 12.

In directing the principles of treatment, Dr Burnett points out three different stages of the fever, (exclusive of that of convalescence in those who recover,) in which the indications of cure are tolerably distinct. The first stage he considers as characterized by local excitement in the head. This stage commonly occupies the first three days of the fever, though sometimes less, and continues till the commencement of gastric symptoms, or the appearance of the yellowness of the skin. "At this time," he justly remarks, "it is a disease simple in its nature, and easily to be managed; but if allowed to run on to disorganization uncontrolled, or if, by improper treatment, the inflammation and congestions be increased, it soon arrives at that stage, when the utmost powers of medicine will be exerted too often in vain." At this time, notwithstanding the prostration of strength, the anxious countenance, and the oppressed pulse of the patient, and even the tendency to syncope on bleeding from the arm, he maintains that a free bleeding from the temporal artery, aided by a purgative, often at once puts an end to the disease.

"The headach, if not entirely removed, is greatly ameliorated by the abstraction of blood from the temporal artery; in many instances so immediately, that the patient has declared, he felt the pain escaping with the blood.—If before this evacuation, the pulse should have been oppressed, it will rise under the lancet; and patients who have been led or carried into the hospital or sick-birth (so great has been the apparent debility) have, after the loss of thirty ounces of blood, risen and walked about, expressing their surprise at their former condition."—p. 21.

This relief, however, is not always permanent: the patient must

must be carefully watched, and on the return of the headach, heat, vascular excitement, &c. the remedy must be repeated in the course of an hour or two. "The great object," Dr Burnett says, "is the removal of the local affection of the brain, or other organ, and the production of a complete remission of the febrile symptoms, in the least possible time, by which the dangerous symptoms of the latter stages are prevented or greatly mitigated, and a perfect and rapid recovery insured."—p. 22. In addition to this plan, Dr Burnett administered purgatives containing moderate or even small doses of calomel, saline juleps, and any cool aqueous drink; and allowed as few bed-clothes as possible on the patient's bed, advised free ventilation, and the use of the cold or tepid affusion. With respect to the last-mentioned remedy, however, he adds, after admitting its great efficacy as an auxiliary, "but the young practitioner must not confide in cold or tepid affusion for the cure of this disease;" they are never of themselves to be depended upon.—p. 26. In this stage he deems the use of emetics injurious, especially in severe cases; and the extreme irritability of stomach, which characterizes the more advanced stages of the fever, is, as might be expected, much increased by emetics, and with it the whole of the dangerous symptoms. It is unnecessary to add, that, in this stage, every stimulant is highly deleterious.

In the second stage of the fever, which intervenes between the first appearance of the severe gastric symptoms and yellow suffusion and the occurrence of nervous symptoms, (the duration of which is various) much is still in the power of medicine; but even this will depend greatly upon the preceding treatment. If the disease have been treated as an inflammatory affection, the symptoms are commonly moderate; if "as a typhoid affection" (by which we presume the author means, with bark and stimulants) they are as generally severe. If the headach is present in this stage, with flushed countenance, suffusion of the eyes, and a firm pulse, a small quantity of blood taken from the temporal artery, while the pulse is carefully examined, will relieve the symptoms: a blister applied to the head at this time is manifestly beneficial, and daily evacuations of the bowels should be procured by gentle laxatives, for the powerful cathartics must be now laid aside; castor oil or purging enemata are recommended. The irritability of the stomach, which is often distressing at this period, is relieved by the effervescing draught, and by the application of leeches, or of a large blister to the epigastrium. A degree of stupor sometimes supervenes in this stage, which is often removed by a blister to the nucha or forehead, or by leeches to the temples. If there is any obvious affection of the

the abdominal viscera, which, the author suggests, should be carefully inquired into, blisters and the warm bath should be resorted to, and even bleeding, if the urgency of the symptoms and the condition of the patient render it expedient. And here the author says,

"I wish particularly to impress the utility of the warm bath; under these circumstances, it will often save the patient, when, from his other symptoms, bleeding would produce fatal consequences. It is obvious that bleeding must not be practised, where the patient is much exhausted; but the warm bath, blisters, and large frequently repeated emollient clysters, will be of the utmost benefit. When the morbid heat is considerable, and the patient exhausted from the severity of the gastric symptoms, or a protracted disease, great advantage will be derived from the use of the warm or tepid bath, or sponging the body."—p. 29.

These observations will satisfy the reader, who, in the midst of his apprehensions about debility or putrescency (for even this *chemical* notion of disease is not yet entirely vanished) might shudder at the idea of using the lancet, that the author has employed it with proper caution and discrimination. He now comes to the third stage of the fever, which includes the period from the accession of nervous symptoms until the termination of the disease in death or convalescence. This stage is marked by the increased uneasiness about the epigastric region, ischuria, singultus, or vomiting of a matter resembling coffee-grounds, &c. The author's observations on this stage, being brief, we shall give in his own words.

"We are now arrived at the most dangerous and distressing stage of the disease, where unhappily the powers of medicine are of least avail; the pulse begins to sink or intermit; and the nervous symptoms become urgent, constituting the third stage. Little more can now be done, than to look on, and endeavour to obviate symptoms as they occur. Singultus is a dangerous, and commonly a most harassing symptom at this time; it will often be relieved by camphorated julep, to which may be added opium and æther. Musk is also sometimes of service, but my experience does not warrant me to speak decidedly. If the pulse sink, the stimuli must be increased; and, under these circumstances, I have found the carbonate of ammonia, with aromatic confection, of singular benefit: but while we endeavour to restore the balance of the circulation, *care must be taken not to induce a state of secondary excitement*, and as the pulse rises, the stimuli should be decreased. Constant attention must still be paid to the daily evacuation of the bowels; but at a period, when the excitability of the system is nearly destroyed, powerful cathartics will be attended with the most deleterious consequences: clysters are particularly serviceable at this time. As the disease

ease advances, the secretions are at times voided involuntarily; in a few I have observed a retention of urine, and in these last cases the catheter should be used; but as a general symptom, there is far oftener a deficiency in the secretion of that fluid. Frequently in this state the stomach rejects every thing: we may now safely indulge the patient *moderately* with any thing to which his fancy leads him. Bottled porter, wine, and brandy and water, have been found beneficial; but no remedy can be relied on with any degree of certainty: whatever calms the irritability of the stomach, and moderately supports the excitability of the system, is useful. A few spoonfuls of arrow-root or sago, with wine and spice, given occasionally, will often be retained by the patient, and greatly at this period assist the cure."—p. 29, 30.

That the system sketched out above is a rational method of cure for fever under all circumstances, (the activity of the remedies being of course proportioned to the climate, constitution, and violence of the disease) those who have had many opportunities of treating fevers, and who have ventured to think for themselves, will, we doubt not, readily admit. It is in the beginning only that any decided influence upon the disease can be produced by physical means, so as to arrest its progress, or materially abbreviate its course. What is at first only morbid action, and somewhat under control, soon becomes partially morbid structure, which time only, and the removal of obstructions and impediments to the natural work of the system, can restore to its natural state. And under the plan laid down in the foregoing sketch of Dr Burnett's practice, this view of the subject appears to be correctly pursued; and that it was *successfully* pursued, seems to be proved from a considerable series of evidence in detail in the after-part of the work.

After stating briefly the cautions requisite during the period of convalescence, Dr Burnett proceeds to notice some remedies, which are commonly associated with the idea of fever, and respecting which his observations accord very accurately with our own experience. With respect to *emetics*, he confidently asserts, that in the Mediterranean fevers, when they are given so as to produce full vomiting, they have been attended by very unfavourable consequences, increasing the headach, sickness, and local affections, and have seldom or never succeeded in removing nausea, and have aggravated, or even prematurely excited the gastric symptoms. *Sudorifics* have never appeared to the author to be productive of the smallest benefit, especially when employed in the early stage. They often fail to induce any perspiration; and, on the other hand, the patient is often covered with sweat at the commencement of the disease, which produces no alleviation,

viation. Antimonial powder assists calomel in keeping the bowels open in the early stage, but is inadmissible whenever there is irritability of stomach. And as to *cinchona*, when given while any pyrexia remained, it was attended with very mischievous effects.

"Under its use, mortality has been great, relapse frequent, and (as in the cases of the Temeraire and Invincible) dysentery attacked nearly all the patients, who had fever in a severe form; nor was there an instance, as far as I could learn, that, when given during a supposed remission of the symptoms, it prevented a return of the paroxysm. Too often it has been given with wine at the commencement of this disease, when the tongue has soon put on a brown, dry, and furred appearance. The anxiety, delirium, and irritability of stomach have been much increased; the whole train of nervous symptoms soon became formidable, resisting every means of alleviation, till death has put a period to the sufferings of the patient." p. 34.

Dr Burnett affirms, that mercury employed in the early stage of this fever in any other way than as a purgative, has not been attended with any benefit. But in long protracted cases, where, from allowing the first stage of the malady to proceed uncontrolled, a morbid affection of the brain or other viscera had ensued, and especially where effusion had apparently occurred within the cranium, he had observed very obvious good effects from the use of mercury in small doses.

After detailing these practical observations and precepts, the author gives a summary sketch of the appearances found on dissecting the bodies of those who had died of the fever; and these very strongly corroborate the views above exhibited. The vessels of the brain were generally distended, and in many instances completely gorged with blood; the membranes were considerably inflamed, often presenting what the author calls a blood-shot appearance; and depositions of coagulable lymph presented themselves among the convolutions. There were occasionally adhesions, and the ventricles were often distended with fluid, sometimes limpid, sometimes yellow. Appearances of high inflammation presented themselves in the cavity of the chest, affecting the lungs, pericardium, and diaphragm, and connected with depositions of lymph and effusions of serum. In the abdomen, the liver was generally found enlarged, and sometimes exhibiting marks of inflammation; the stomach distended with air, more or less inflamed, and containing a dark-coloured matter, and the intestines in a similar condition, with frequent intus-susception.

After having followed the author through his general details of the principles of treatment, and his views of the nature of the fever, which occurs in the autumnal season on the shores of the Mediterranean,

Mediterranean, we cannot undertake to give any abstract of the numerous cases, which he has recorded in evidence of the truth of these views, and of the success of the practice founded upon them; nor can we enter into the history of the recurrence of this fever during successive seasons, which he has given at length, aided by documents from several of his brethren. For these particulars we must refer the reader to the volume itself. With respect to the nature of the epidemics, which have occurred of late years at Carthagena and Gibraltar, the testimony of Dr Burnett is strongly confirmed by the communications of other witnesses to the progress of these fevers, and tends to demonstrate their identity with the fevers that have almost annually occurred in the ships upon the Mediterranean stations, especially when exposed to the land air, under particular circumstances. He enters at considerable length into the question respecting the contagious or non-contagious nature of these fevers, and concludes, from what he deems "powerful evidence," "that it is neither propagated by persons or goods, but is the genuine product of miasma," (p. 270), and hence that the establishment of quarantine, with the view of preventing the propagation or introduction of such a fever, is as unnecessary as it is unavailing.

III.

A Practical Essay on the Diseases of the Vessels and Glands of the Absorbent System; being the Substance of Observations which obtained the Prize for 1812, *offered by the Royal College of Surgeons in London: To which are added, Surgical Cases, with Practical Remarks.* By WILLIAM GOODLAD, Surgeon, Bury, Lancashire, Member of the College, &c. London, 1814. pp. 228.

WE consider the examination of the structure, functions, and diseases of particular organs, in a connected and systematic manner, as certainly leading to the improvement of our science. They mutually throw light upon each other; and, what is perhaps of equal importance, they prevent the promulgation of erroneous doctrines, which might naturally arise from considering them exclusively in one point of view, or from a single apparently accurate observation, when directly contrasted with the results of varied and general

general experience. The profession is therefore highly indebted to the London College, for the selection of such subjects as prize-questions; for good must arise from directing the attention of many to one point, even though no single essay should fulfil our expectations. Mr Goodlad treats in succession of the Anatomy of the Absorbent Vessels, of the Chemical Properties of Chyle and Lymph, and of the Diseases of the Absorbent Vessels and Glands; to which he has added some surgical cases, with remarks. The two first chapters we shall pass over without further notice. The absorbent vessels may become inflamed, varicose, ruptured, wounded, or obstructed. Mr Goodlad treats in succession of these diseases, and of their treatment; and, so far as the subject is known, he does it ably and distinctly. It must, however, be obvious, that, as absorbents exist everywhere, they must always be involved in the diseases affecting any part of the body. Thus we cannot conceive any considerable wound, which shall not open lymphatic vessels as well as veins, or any local inflammation in which they shall not be concerned. But cases in which the lymphatics are chiefly, or solely affected, are of rare occurrence, and difficult observation. We have, however, seen a very trifling ulcer of an extremity prove exceedingly troublesome and untractable, by the great quantity of lymph which it discharged, leaving no doubt as to its source. There is also a very singular case related by Mr Maxwell, in the fifteenth volume of the Medical Commentaries, which most probably arose from a rupture of a lymphatic trunk, although the dissection of the patient did not clear up the nature of the disease.

The diseases of the absorbent glands, of which our author treats, are inflammation, ossification, and scrofula; the last subject, in particular, he has discussed at great length. He divides scrofulous affections of these glands into three *species*: in the first, and more frequent species, an early effusion of fluid is perceptible, which is exterior to the body of the swelled gland, and is contained in, and circumscribed by, the adjacent cellular membrane; the second is attended with simple enlargement of the glandular substance, or of the adjacent parts; in the third species, which rarely occurs, the abscess is formed in the substance of the gland, and if the cells of the gland are separated by adhesion, each cell may contain an abscess, and successive openings are formed for their discharge.

We pass over our author's remarks on the theory of scrofula, and his strictures upon the opinions of preceding writers, with merely observing, that he considers scrofula as a simple chronic inflammation, having nothing specific in its nature, and inde-

pendent of any morbid change of the fluids. His treatment of the disease accords with this theory; and his directions, upon the whole, seem so judicious, that we are tempted to condense them. To prevent the occurrence of the disease in children, whose frames are weak and irritable, he recommends attention to the strengthening of the constitution, and the removal of any cause of inflammation, by a due use of animal food, the abstaining from hot slopping liquors, a proper attention to the bowels, warm clothing, moderate exercise, plenty of sleep, with light covering, and the shower-bath, succeeded by friction. If an enlarged gland has already appeared, a fur of any kind may be worn round the neck during the day, and a hare's skin fastened to the night-cap, and fitted to the part, may be substituted in the night, and worn with the fur side in contact with the skin. Cooling washes, and the early and frequent application of leeches, must be combined with the general treatment, as his object in this stage of the disease is to diminish and remove the inflammatory action. The surgeon must, however, be upon his guard, not to push these remedies too far. The cooling washes are intended to subdue increased arterial action, and to regulate the temperature of the part; not to reduce it below the healthy standard, or to induce shivering. It is by their continued use that they will take off the disposition to enlargement. Too great a degree of cold will derange the constitution, especially in weak habits; and if the glandular enlargement be considerable, destroy the power of the part, so as to give it a tendency to slough off. For similar reasons, the local blood-letting must never be excessive, though frequently repeated.

When the progress of the tumour is arrested, we must attempt its absorption by stimulating applications, care being taken that reaction is not produced. This object is well attained by the use of a solution of sulphate of zinc, in the proportion of half a drachm to three drachms in eight ounces of water, which, along with the cooling effects of evaporation, keeps up a gentle stimulus on the surface, using at the same time regular friction with the bare hand, or with some stimulating liniment, and sometimes electricity, along with small doses of calomel night and morning. If there should appear the least disposition to enlargement, the stimulating plan must be suspended, and recourse had to the application of leeches.

"Should the abscess increase, notwithstanding our efforts to prevent it, the glandular swelling with which it is accompanied, will regulate its future treatment. When it is formed of simple effusion, and is uncombined with enlargement in the gland, or when that enlargement is not considerable, the most beneficial practice will be, I

believe, to evacuate the matter: for if we wait until the skin be ulcerated, the cellular membrane, having been so long exposed to diseased action, will ulcerate or slough, and the death of the skin must inevitably follow.

"As soon as we are convinced that the abscess will continue to enlarge, it appears proper to puncture it, because the adhesions by which its boundaries are circumscribed are so slight, that a trifling pressure destroys them. The abscess extends laterally therefore, instead of approaching the surface, and as the whole cavity sloughs on exposure, the isolation and death of the skin, and the size of the ulcer, will be proportioned to these circumstances."

"By an adherence to this practice, a further and more important advantage may be obtained, I mean the prevention of ulceration in the cavity of the abscess, and its obliteration by adhesion. Because, though I believe inflammation exists in every period of the disease, yet it cannot be denied that it becomes more considerable as the ulcerative action is more extended. In proportion to the inflammation, is the chance of destruction in the cellular membrane; the matter should therefore be evacuated before this increased action has taken place, and whilst the cells retain sufficient vigour to support their life, and to produce reunion. It is true, that in whatever manner, or at whatever time, the abscess is opened, ulceration will follow; but if the inflammation has not been considerable, it may generally be prevented from increasing, by continuing the cold application, after the abscess is evacuated.

"I know not what opinion may be formed of this theory, but the benefit of the practice I have often witnessed, and I can therefore confidently recommend it. An abscess, which, if allowed to ulcerate would occupy many weeks or months, may by this treatment be obliterated in a few days. It is not perhaps easy to account for the difference in the effect of the same application, when applied to an abscess with the skin entire, and to the cellular substance, in which the abscess is formed: in the one case, it shall not arrest its progress; in the other, the discharge will immediately cease.

"My first trial of this treatment was in a girl about twelve years of age, who had a small abscess between the metacarpal bones of the little and ring finger, on the back of the hand. It had been preceded by inflammation, to which cold washes were applied, and when the abscess had approached the skin, it was punctured; the cold stupes were continued, and in three days the abscess was obliterated."

Mr Goodlad is so confident of the success of the sulphate of zinc in solution, for procuring the obliteration of scrofulous abscesses by adhesion, that he does not hesitate to declare, that he should consider a surgeon unpardonable who had an extensive scar from them, when unattended with glandular enlargement. In one case when his favourite remedy was applied in solution,

"the

" the aperture healed, but the abscess filled, and its contents were repeatedly discharged.

" An ointment of tartarized antimony was well rubbed upon the surface, but though it excited considerable irritation, the contents of the abscess were not at all reduced, nor did it approach the surface. On being again punctured, a strong solution of the sulphate of zinc was injected into it, and retained about ten minutes. It excited little uneasiness, and the parts united by adhesion.

The cases added to the prize-essay, are the case of inguinal aneurism, which the author did us the honour to publish originally in our Journal; a case of gunshot wound, in which the ball passed between the femoral artery and vein, where they pass out of the pelvis, and a cure was performed without any operation, or unpleasant consequences; two cases of congenital hernia; and a singular case, which Mr Goodlad supposed to arise from inflammation of the theca of the vertebral column.

IV.

An Essay on the Absorbents; comprising some Observations upon the relative Pathologies and Functions of the Absorbent and Secreting Systems. By DANIEL PRING, Member of the Royal College of Surgeons, London, and Surgeon at Bath. 8vo, pp. 92. Bath, 1813.

THIS essay was probably composed with the intention of being put in competition for the prize offered by the College of Surgeons, but was not presented, as its author " felt convinced that it was, on many accounts, but ill calculated" to succeed. In this he judged correctly, for it is too little practical, and too speculative. We shall briefly notice the contents of Mr Pring's publication. The sketch of the history of the absorbents is very superficial; and we cannot forgive him for omitting to mention MONRO as one of those to whom we are most indebted for our present knowledge of the absorbent system. From his " cursory view of the anatomy of the absorbents," we extract the following passage, as exemplifying our author's manner:

" We are informed by Ruysch, Morgagni, and some others, that the glands of the mesentery *entirely* disappear in old age. Although we cannot choose but repose great confidence upon the respectability of such

such authority, individual observation must nevertheless invalidate the assertion as an universal law.

"It would most probably prove a fruitless research, to inquire how far that state of the body which marks the period of advanced years is connected with, or attributable to the destruction of the glandular system of the mesentery. Experience and observation may throw some light upon the question; but it is a point, the determination of which hardly falls within the sphere of practicable experiment. The instances of the disappearance of these glands are irregular; and where the opportunities are abundant, it would be a series of observations not altogether devoid of interest, or perhaps the higher incentive, utility, to mark the different states of body which are associated with such glandular varieties."

The next section is "on some particulars of the physiology of the absorbent system." It contains a curious speculation on the source of the colour of the blood.

"The new matter of the body is supplied from the intestines; and the chyle in the absorbents of the mesentery is white: whence then does it derive its change of colour? It is said to be produced by its mixture with the blood. But the blood is *totally* renewed within a definite time; and if the alteration in the colour of the chyle is assignable to this cause, in proportion as the blood is wasted and renewed, it ought to become progressively pale until it had attained to the whiteness of the chyle itself. We must look then for some *source* of colorification—and where will it be found?"

"It is mere matter of conjecture that this function resides in the lungs: we are warranted to infer that it exists somewhere; but it is the business of future experiment to determine to what organ it belongs. It is an interesting, and I think will one day prove a highly productive theme of inquiry, to investigate the physiology of the colorification of the animal fluids, beginning from their first formation in the mesenteric absorbents, and tracing from thence the varieties of colour incidental to them in the different orders of circulation.

"In the course of this inquiry we should notice, first, the change of chyle into arterial blood, and we should be highly interested in the explanation of the manner and means by which this change was accomplished. We should next remark; that some of the arterial blood flowed to the remotest parts of the body, and still retained its florid red, and all the other properties distinguishing it from venous blood. On the other hand, we should again observe it becoming black, and assuming the properties and appearance of venous blood, when it had proceeded but a very limited distance from the heart."

The change of arterial blood into venous, our author ascribes to the separation of a part of its constituents, by the exhalent arteries; and the conversion of venous into arterial blood, to the re-combination

re-combination in the lungs, of the abstracted constituents conveyed, by the absorbent system, into the sanguineous circulation near the heart.

Mr Pring, in treating "on the agents of absorption," satisfactorily refutes the conclusion inferred from some recent experiments made by Mr Brodie of London, and Dr Magendie of Paris, that absorption takes place exclusively from venous orifices, and that the absorbents themselves have no participation in the process. We acknowledge our obligations to Mr Pring for checking the spreading heterodoxy upon this subject. Much as we admire and encourage all attempts to advance physiology by experimental investigation, there is perhaps no science whose progress has been so greatly impeded by partial experiments, and hasty inductions. Independently of the chance of error in these inductions, there are so many circumstances which affect the result, that we should very rarely draw positive conclusions from single experiments, however ingeniously contrived, or dexterously performed. Still more must we distrust their results, when they are inconsistent with general observation, or even the doctrines generally taught in the schools, without being satisfied that we are mistaken in the former, and that the grounds upon which the latter were advanced are untenable. But, in this case, upon reverting to the facts and arguments upon which the function of the absorbents, as commonly understood, was established, we shall be convinced that all experiments, apparently inconsistent with the generally received opinion, must admit of an explanation reconcileable with it.

To account for the *modus operandi* of the absorbents, Mr Pring supposes them to possess a pulsatory action, and proceeds to speculate upon the cause of their alternate contraction and dilatation. But pulsation in the absorbents is not supported by observation, and is even inconsistent with their valvular structure, and their analogy with the veins.

The general pathology of the absorbent system is very briefly discussed. They are subject to diseases of action, and diseases of structure: The former consist in deficiency, or excess of excitement; the latter "become operative only by establishing one or the other of the conditions incident to the former."

"The diseases of the absorbents must frequently arise from an idiosyncracy or pristine peculiarity of constitution, excited to the establishment of morbid conditions, sometimes without any assignable cause, but merely from a continuance of the actions by which the life of the animal is sustained."

In

In the remaining section, Mr Pring treats of scrofula * and emaciations, scirrhus and fungus hæmatodes, dropsical affections, and some diseases of the bones, as connected with the absorbent system,—of the relation between the absorbent and secreting systems, in relation to disease,—of the inflammation of the absorbents,—and of the means of exciting the absorbents into action.

" Secretions and effusions are the consequences of increased vascular actions. The violence of these actions is diminished by the employment of the means termed antiphlogistic. When the excitement is reduced to the standard of health, the natural relations of the secreting and absorbent powers are restored. But, in order to fulfil the indication, it is necessary that the energy of the latter should prevail, and exceed that of the former. Leeches and evaporating lotions are inadequate to this design; such means are best suited to it as effect the double purpose of restraining vascular excitement, and of increasing, at the same time, that of the agents of absorption.

" In the whole catalogue of the means conformable with this view, there are, perhaps, none more decidedly efficient than blisters. They diminish the excess of vascular action upon the principle of counter-irritation; and although in this particular they may be supposed to have, on general principles, a similar effect upon all the parts of the structure, the absorbents are nevertheless stimulated to an increased activity, by the immediate application of cantharides to their own surfaces, in consequence of the function which they specifically exert.

" The benefit of some other means, as lotions containing the crude ammoniacal salt, vinegar, &c. may be explained in the same way: they reduce the temperature of the surface, and thereby diminish arterial action; while the stimulating properties which give effect to their use, increase the energy of the absorbents through the medium of absorption. The very frequent consequences (affecting particularly the urinary organs) which result from the employment of blisters, are a sufficient proof of the facility with which cantharides are absorbed."

" My experience does not inform me whether a perpetual and alternate blistering of the abdomen and loins would, in the case of scrofulous enlargement of the mesenteric glands, be attended with the same happy result. The principle, however, is here admissible; the

* Mr Pring's general indications for the treatment of this complaint coincide with Mr Goodlad's.

" When, however, those glands are attacked by it, which merit the attention of the surgeon, two indications must arise, from the varieties by which its progress is characterized. The first is to diminish the local actions, which, though of a specific kind, are nevertheless inflammatory; the second is to produce the absorption of adventitious matter, when the increase of excitement has subsided." Mr Pring, p. 54.

the effect is to be determined only by the practice. It is probable that the means would fail, not because the design is defective, but rather owing to the remoteness of the disease from the surface. It may be said, by way of suggestion, that the internal exhibition of remedies with the same view is not altogether precluded."

It is evident, from the perusal of this essay, that it has been hastily composed, and prematurely published. Mr Pring possesses talents, and writes with animation; but he is much too confident in his powers of reasoning; and his style is diffuse, turgid, and incorrect.

V.

Observations on the distinguishing Symptoms of three different species of Pulmonary Consumption, the Catarrhal, the Apostematous, and the Tuberculous; with some remarks on the remedies and regimen best fitted for the prevention, removal, or alleviation of each species. By ANDREW DUNCAN, Senior, M. D. Physician to his Royal Highness the Prince Regent for Scotland, Fellow of the Royal College of Physicians of Edinburgh; Professor of the Institutes of Medicine in the University of Edinburgh, &c. 8vo, Edinburgh, 1813.

THE author of this Treatise is well known as one of the most laborious cultivators of the healing art. As a teacher of medicine, and as editor of the Medical Commentaries and Annals, he has been led to study and record every important fact, and every improvement in medicine, for a period of more than half a century. But he has not been merely the historian of what others have done; he has had many opportunities, both in public and private practice, of bringing every rational scheme or discovery to the fair test of experiment. Some of the results thus obtained, he has now submitted to the public.

After a short address to the Prince Regent, and a letter of the same description to his friend and former pupil, Sir Gilbert Blane, the author gives the following account of the nature and objects of his present work;

"I

" I here present to the public a very short treatise, on a very important subject. With little difficulty, I might have extended it to a much greater length A particular relation of even a selection of those cases from which my observations have been deduced, might have filled a large volume. My object, however, has been to lay before the intelligent reader, not the detail, but the result of my practice, and to state merely the conclusions I have drawn from the facts which I have witnessed in watching the progress of pulmonary consumption."

The author next adverts, in a short introduction, to the question which has been so much agitated, whether phthisis is to be regarded as an idiopathic or a symptomatic disease; and concludes by observing, " that there can be no doubt that it may justly be considered as an important genus by itself."

From the endless modifications of hectic, or that form of fever which always attends pulmonary consumption, nosologists have been led to consider the species as very numerous. Sauvages, for instance, reckons up no less than twenty. But this frivolous minuteness can only serve to distract the attention of the beginner, and bewilder even the most experienced. Without presuming that he has embraced the whole, the author has confined his attention to three species, which may be easily distinguished from one another, and each of which requires a peculiar mode of treatment. On this division, and the principles which led to its adoption, he makes the following remarks:

" In the division of any genus of disease into species, two great objects are to be had in view. The first and principal object is to point out such species as require a different mode of practice from each other, by obvious symptoms, during the life of the patient. The distinction of species, which can be discovered only by dissection after death, serves merely to perplex and puzzle, without being of any material benefit. On this general ground, and with these objects in view, in phthisis pulmonalis, three different species may be pointed out, which may not only be distinguished by obvious symptoms, during the life of the patient, but which also require some difference in that mode of treatment best fitted for the removal of each.

" These species may be distinguished by the titles of the Catarrhal, the Apostematous, and the Tuberculous phthisis. The principal circumstances between these, requiring variety in the mode of treatment, is the difference in the source from whence the purulent matter is derived. In the first modification it is merely separated from an inflamed surface, in a manner similar to the separation of pus from a blister-issue. In the second modification, it is furnished from an abscess of a considerable size, which may be formed in the cellular substance of the lungs, as well as of any other part of the human body. In the third, it is furnished from a tubercle, or from the parts surrounding

rounding a tubercle, terminating in suppuration, and commonly yielding, not proper purulent matter, but rather an ulcerous sanies, somewhat resembling that which is often yielded by lymphatic glands, in cases of scrofula, when they terminate in suppuration."

The remainder of the work is divided into eight chapters, which we shall shortly notice in their order.

I. " A description of the pneumonic symptoms occurring in catarrhal phthisis. In this chapter, the author describes with great minuteness, the almost imperceptible changes by which a common catarrh may degenerate into a case of confirmed phthisis, and concludes with an account of the means we possess of ascertaining the quality of the matter expectorated.

II. " A description of the pneumonic symptoms occurring in the apostematous phthisis." The author distinguishes the present form of the disease from the one previously described; inquires into the various causes which most generally give rise to it, and exposes with propriety the incorrectness of Dr Cullen's notions, who regarded it as a mere consequence of hæmoptysis. The following short account of the symptoms and progress of this species deserves to be noticed:

" In most instances of the apostematous phthisis, the first symptom of the affection is a fixed pain in the breast. This pain is not indeed in general excruciating, but it is almost constantly felt to some degree, and is commonly referred to some particular spot. For the most part it is rather a gravative than an acute pain; but it is not unfrequently of the pulsating kind. Whatever the nature of the pain may be, it is very commonly attended with some degree of dyspnœa. With that dyspnœa the patient is sensibly affected, even when in an erect posture; but it is in general much increased in a horizontal position; and it is often particularly aggravated when the patient lies on one side, rather than the other. Often it happens that the patient can breathe only when he lies on that side on which the apostema is situated; the reason of which is abundantly obvious. With this state of respiration, there occur frequent and severe fits of coughing; but for some time these are attended with very little, or even no expectoration.

III. " A description of the pneumonic symptoms occurring in the tuberculous phthisis." This is the most common, and the most incurable form of pulmonary consumption, and unfortunately the least alarming; so much so, indeed, that the disease has generally arrived at an advanced stage before danger is at all apprehended. The patients themselves have no fears, the practitioner is often deceived, and even when he is not deceived, it is so ungracious a thing to forbode evil, that this part of his duty is seldom discharged with that fidelity, or his admonitions received

ed with that attention, which their importance deserves. After some pathological remarks on the origin and nature of tubercles, the author gives the following account of the symptoms which characterize the incipient stage of this species of consumption:

"Of all the modifications of phthisis, the pneumonic complaints which occur in the tuberculous are the least alarming. Hence this species often subsists for a considerable time before it awakens the attention of the patient. This the rather happens, because the fever with which the tuberculous phthisis is attended at its commencement, is in general accompanied with high spirits. And from this state of exhilaration, even to the latest periods of the disease, patients affected with tuberculous phthisis have often no apprehension of their own danger. At the commencement of tuberculous phthisis, neither the cough nor dyspnœa are by any means urgent; and in many instances, even to the very end of the affection, there hardly occurs any expectoration. The cough in general is of the short tickling kind, without being violent; and may be termed rather a tussicula than a tussis. But while it takes place without any great uneasiness, it is still troublesome, from being very frequent. Notwithstanding, however, these frequently repeated efforts towards expectoration, the irritating cause is not removed, and those slight fits of coughing, terminating without the smallest expectoration even of mucus, are again speedily renewed. In most cases no remarkable pain of breast attends the tuberculous phthisis; and when pain occurs, it is neither fixed to any particular spot, nor is it constant. In many instances no dyspnœa whatever occurs in the tuberculous phthisis; or at least difficulty of breathing is observed only upon motion or exertion. When the patient remains at rest, the breathing is perfectly free, and it is very little, if at all, affected, by change in the position of the body. Contrary to what happens in the apostematous phthisis, the patient can lie with equal ease on either side."

Though we are highly pleased with the distinct and intelligible account, which the author has given of the three species of phthisis, yet we cannot help regretting, that his plan did not admit of a more detailed account of the symptoms of a disease, so exceedingly diversified, and where success in practice, if at all attainable, depends on early and accurate discrimination. The outlines are strongly marked, but some of the more minute features are omitted.

IV. "Observations on the hectic fever in phthisis pulmonalis, attending the second stage of the disease." This form of fever is a constant attendant on pulmonary consumption, and common to all the three species. The author, after shortly adverting to the different theories respecting its origin and nature, proceeds to give a detailed account of its symptoms. This account is more remarkable

remarkable for fidelity, conciseness, and perspicuity, than for any thing new.

V. "Observations on the symptoms occurring in the last stage of phthisis pulmonalis; what may be termed the supervening or consequent symptoms." These are, emaciation, debility, a deficiency of fluids in the extreme vessels, loss of hair, incurvated state of the nails, cessation of the menses, aphthous affections of the mouth and fauces, colliquative sweats and diarrhœa; and lastly, delirium, which generally closes the scene. The author gives a very plain and obvious account of the origin and nature of these symptoms, or what may be termed their proximate cause. He also points out, with great propriety, the danger arising from directing the attention to one or two symptoms alone, without regarding the diseased habit on which the whole depends.

VI. "Of the diagnosis in phthisis pulmonalis." This chapter is naturally divided into two heads. The means of distinguishing this disease from others, and the means of distinguishing between the different species. Under the first of these heads, the chief difficulty arises in drawing the line of distinction between pulmonary consumption and common catarrh. Almost every case of phthisis is, for a longer or shorter period, taken to be a common cold, and in a majority of instances it is really so. It becomes, therefore, a nice, but an important point in practice, to say where the one ends, and where the other begins. The author's long experience and habits of minute observation, have enabled him to do this with more than ordinary clearness and precision. As his remarks on this head, and the different species, are too long for quotation, and do not readily admit of abridgement, we must refer the reader to the work itself.

This concludes the first part of the esssay, which is devoted chiefly to an account of the symptoms, causes, and nature of the disease. In the two last chapters he takes up the treatment. In the first of these, he discusses those general or leading principles, which ought to guide our practice on all occasions; and in the last, gives a more minute detail of such individual methods of treatment, as he has found most successful.

With regard to the general principles, he remarks, that the practitioner has, for the most part, one of two objects in view; to effect a radical cure, or to alleviate distressing symptoms. The first of these is to be our grand object during the continuance of the incipient stage; the last, when there is no longer hope of a final recovery. In the catarrhal species, our objects are, to correct or restrain the purulent secretion from the inflamed surface, to heal up that surface, and to bring on a healthy action of the vessels. In the apostematous, to discharge the matter already

already collected, to prevent or moderate the formation of more, and to aid nature in healing up the ulcer. In the tuberculous, to effect the resolution of the inflamed tubercles, before the suppurative process begin,—after suppuration, to improve the quality of the matter secreted,—and, finally, to clean and heal up the ulcer. This last indication, as well as some of the former, the author very candidly admits, is not easily fulfilled.

The concluding chapter consists of "Observations on particular practices employed in phthisis pulmonalis, and which have been recommended by eminent writers." On this head, it may be remarked, that the author, having no favourite plan of treatment of his own, is the more likely to give a fair and candid trial to those of others; for, against the bewitching influence of theory, in the observing and relating of facts, the most upright principles are not always a sufficient protection.

The author complains of the difficulty of giving any thing like a complete view of the different modes of treatment in consumption. "From what I have seen in actual practice," says he, "I might say much, both in confirmation, and in refutation of the sentiments of others, respecting particular remedies. But my object is to deliver, not the detail, but the result of my own observations; and I shall, therefore, confine myself to a few remarks on what I consider as the most important modes of cure, either from their beneficial or prejudicial effects."

With respect to digitalis, a remedy which has been so fashionable, and so much extolled of late years, we shall give the result of the author's observations in his own words, and we do this the more readily, as it corresponds completely with our own:

"The fox-glove has unquestionably a wonderful influence on the state of the pulse. Under proper management, it produces a slowness of the pulse not perhaps to be obtained from any other medicine yet discovered. In phthisis, however, the pulse is often quickened to a remarkable degree. And it has been the opinion of some, that, by reducing it to the natural standard, from the use of digitalis, consumption may be overcome. But it is almost unnecessary to observe, that the quickness of pulse in phthisis is merely symptomatic, and that the reduction of it even below the natural standard can have no effect, either in removing a tubercle, or in curing an ulcer in the lungs. Hence, on this ground, it is vain to expect a radical cure from its use. That in certain cases, however, it may be employed with advantage, I am very far from denying.

"Besides in general diminishing the celerity of the pulse, the fox-glove, in most instances, also operates as a very powerful diuretic. It is, however, well known, that a free discharge of urine has often great influence in relieving the breast. In this way it may be highly useful in catarrhal phthisis; and a diminution of the pulse may certainly somewhat alleviate the hectic fever. But I am sorry to say,

that,

that, for my own part, I have not witnessed any great benefit from digitalis in any case of phthisis; and I have seldom been able, by means of digitalis, so far to diminish the celerity of the pulse in phthisis; as to bring it even to the natural standard. It would seem, that, in this disease, such is the influence of the causes quickening circulation, that even the power of the digitalis is not able to counteract them."

As to sea voyages, the author has found them useful in the early stage of the tuberculous consumption; in the other species, and in the confirmed state of all, they generally prove hurtful. He makes nearly the same remark on the removal to a warm climate. "The greater part of those patients who have had recourse to this practice, after phthisis has been distinctly marked, have only gone to breathe their last in a foreign climate."

The regimen of the patient is often a most important part of the treatment in phthisis; but as no one kind of regimen can be applicable to all the species or stages of the disease, it requires as much discrimination to regulate this department of practice as to direct the use of medicines. The author has discussed this head of the subject at considerable length, together with the remedies usually employed to relieve the urgent symptoms, which occur towards the fatal termination; but having already extended this article beyond our original intention, we shall conclude the analysis by quoting his account of a new anodyne, which he has found to be very useful in alleviating the cough that always attends, and is often a very distressing symptom, in the advanced stage of pulmonary consumption.

"Where opium cannot be employed, different articles of this kind, particularly the preparations of Hyosciamus niger and Humulus lupulus, have often been used with advantage; but of all the substitutes for opium, which I have ever employed in practice, I have found none from which I have found so much benefit as from the preparations formed from the inspissated white juice of the common garden lettuce. Even since the days of Galen, among the Romans, who employed it as a soporific in his own case, this article, like the Papaver somniferum, and many other plants yielding a milky juice, have been known to possess a power of inducing sleep; but for a long time, although constantly cultivated as a sallad, it has been almost entirely neglected as a soporific. A few years ago, some circumstances drew my attention to this article, and I lately published, in the Memoirs of the Caledonian Horticultural Society, an account of different trials which I had made with it. These I shall not here repeat, but only observe, that of all the medicines which I have employed for alleviating cough in phthisis, and indeed as a sedative in other diseases, next to opium, I have found no article so beneficial as that substance, which, in the memoirs above-mentioned, I have termed Lactucarium."

VI. VII.

Particulars of the Successful Treatment of a Case of Hydrophobia; with Observations, &c. By RICE WYNNE, Apothecary, Shrewsbury. 8vo. pp. 32.

Hints on Hydrophobia. By HENRY EDMONDSTON, A. M. Surgeon. 8vo. pp. 66.

THE first of these pamphlets contains the history of a case of hydrophobia succesfully treated by blood-letting; the second is a popular essay on the means of preventing the accession of the disease in persons bitten by dogs supposed to be mad,—on the diagnosis of rabies in dogs,—and proposals for exterminating the disease, or lessening the frequency of its occurrence; to which is added, a controversial appendix.

In alleged cases of hydrophobia, the first point is to establish the actual existence of the disease. But if this be attended with difficulty when we possess every means of investigating the fact, from the observation of the symptoms in the sufferer, and the examination of all the evidence in relation to the question, how much more difficult must it be, when we have only the description by others to decide upon? In reality, if the observer be certain with regard to the existence of the disease, the reader or critic must give him credit for his opinion, even though the description should be imperfect, or apparently erroneous, unless it betray proofs of ignorance and inattention. Upon these principles, we have no reason to doubt that Mr Wynne's patient actually was attacked with hydrophobia, and that he was cured by blood-letting. He particularly insists upon the necessity of abstracting the blood rapidly, and of inducing syncope. In his own patient,

"Twenty ounces of blood were taken from the arm in six minutes. He fainted; and the pulse could scarcely be felt for one hour. His convulsions abated; his countenance had nearly lost all expression of distress; and, when he recovered, his first request was to be allowed to indulge in that which the bare idea of, but one hour before, seemed to be a source of the greatest suffering. He drank some water, and was greatly refreshed by it. At this period, no medicine whatever had been taken, nor for the space of two hours afterwards, and, during the whole time, he was comfortable, and his bowels were relieved. He now commenced taking, every three hours, large doses of opium, &c.; but, instead of his continuing to enjoy ease and comfort, or further relief from his bowels, his

his night was restless, and in the morning, there appeared to be some reasons for apprehending a recurrence of the disease. Bleeding was again had recourse to, with similar success, excepting, that the same effect was not produced upon the bowels, which possibly were rendered more difficult of action from the use of the opium, but which was afterwards effected by opening medicine. From this time no further symptom worthy of mention occurred, and the patient perfectly recovered."

And in commenting upon Mr Bellingen's case, in which venesection apparently failed, he says,

"Without wishing to shew any disrespect to the opinion of Mr Bellingen, might it not be inferred, that the first bleeding was incomplete, for although the 40 ounces were taken, his patient did not faint, and that he was weakened, and the disease too far advanced, for the second bleeding to have a chance of success?"

Mr Edmondston's publication had its origin in the occurrence of two cases of hydrophobia in Newcastle, which, however, he has not detailed, as they are intended for publication in the Transactions of the Medico-Chirurgical Society of London. There cannot be a doubt of the propriety of informing the public of every thing relative to this disease. On such a subject, the greatest evils arise from ignorance. On the one hand, much mischief is done by false alarm, and on the other, the means of prevention are neglected when the danger is real. Popular instruction can alone remedy these evils, and to attain this end, Mr Edmondston's pamphlet is well adapted. We shall confine our observations to the *Medical Police* of the subject. Mr Edmonston's first proposal relates to the reduction of the number of useless dogs, as it is by the canine race only that the disease seems to be propagated. There is perhaps no plan so well calculated to effect this object, as taxation; and it only requires an extension of the present tax to the dogs of persons not liable to the *assessed taxes*, who are at present strangely exempted, as if the legislature, in imposing it, had only in view the raising of money, and totally disregarded the security of the people. On the contrary, the dog-tax should be strictly exacted from such persons, because, with the exception perhaps of the blind, they have no occasion to keep dogs; and it is chiefly by such dogs, which are not properly looked after, that the contagion of rabies is kept up, and disseminated. The proposal of withholding all parochial relief from applicants who possess dogs, is also well calculated to reduce the number of useless dogs. But to obviate all danger, Mr Edmondston proposes, "that no dog, of whatever description, should be suffered to go at large unmuzzled;" and after pointing out the defects of the common muzzle, he adds,

"But all danger may be guarded against at once, and all inconvenience

to

to the dog almost effectually prevented, by having the muzzle made to project a little beyond the point of the snout. Instead of the fore part of it being made with a thong and buckle, it should be formed of a ring of thick, strong leather, which may be made of a sufficient width to permit so extensive a play to the jaws, that the dog might even eat food presented to him, and would experience no great difficulty in drinking. The muzzle itself would thus be greatly simplified in its construction, and many of the consequences of inattention in putting it on obviated."

All that we shall say of the appendix is, that, as far as we can judge, Dr Trotter was not wrong in visiting Mrs Taylor, even although not done according to strict professional etiquette; because we think that it may be dispensed with upon the occurrence of so rare and interesting a disease as hydrophobia: that, even supposing he was right in believing that the patient was not affected with hydrophobia, in consequence of the bite of a mad dog, which, however, we do not think him, he was wrong in promulgating an opinion injurious to the reputation of her medical attendants, without having consulted with them, and knowing their views of the case: and that the letter which he published in the Newcastle newspapers is so contemptible, that Mr Edmondston is much to blame for having noticed it.

PART

PART III.

MEDICAL INTELLIGENCE.

Report of Diseases treated at the PUBLIC DISPENSARY, *(now Carey-Street,) London, from May 31st to August 31st 1814.*

Acute Diseases.

	No. of Cases.		No. of Cases.
Tertiana	1	Peritonitis	1
Febris	22	Catarrhus	38
—— infantum remittens	13	Hydrocephalus	2
Epistaxis	1	Variola	3
Cynanche trachealis	1	Rubeola	24
Peripneumonia	2	Scarlatina	10
Pleuritis	1	Erysipelas	1
Pertussis	7	Herpes zoster	1
Hepatitis	2	Rheumatismus	3
Dysenteria	15	Abortus	2
Cholera	8	Convulsio	1

Chronic Diseases.

	No. of cases.		No. of cases.
Vertigo	9	Colica pictonum	4
Cephalæa	29	Peritonitis chron.	1
Asthma	1	Physconia	2
Catarrhus chronicus	15	Hæmorrhois	2
Hæmoptysis	21	Diarrhœa	18
Phthisis	24	Constipatio	2
Palpitatio	5	Tænia	1
Gastrodynia	26	Lumbricus	1
Dyspepsia	16	Marasmus	4
Vomitus biliosus	9	Nephralgia	5
Hæmatemesis	3	Dysuria	2
Hepatalgia	2	Hæmaturia	3
Icterus	4	Amenorrhœa	4
Pyrosis	3	Dysmenorrhœa	2
Enterodynia	24	Menorrhagia	8

Chronic

Chronic Diseases continued.

	No. of cases.		No. of cases.
Leucorrhœa	12	Rachitis	1
Hysteralgia	6	Struma	3
Epilepsia	1	Ophthalmia	4
Hypochondriasis	1	Strophulus	1
Amnesia	1	Prurigo	2
Asthenia	21	Lepra	3
Hysteria	5	Psoriasis	5
Chorea	1	Roseola	1
Hemiplegia et paraplegia	5	Urticaria	6
Paralysis saturnina	2	Purpura	4
Anasarca	3	Erythema	1
Ascites	1	Porrigo	21
Hydrops tot. corp.	4	Scabies	14
Rheumatismus chronicus	32	Ecthyma	2
Lumbago	6	Rupia	3

The summer has been moderate, or rather cool, in point of temperature, with the exception of the last week in July, and especially of Thursday the 28th, on which day the thermometer stood at 90°, or 91°, in the shade: it had been at 86° and 88° on the four preceding days. Distant lightning in every direction, on the Thursday night, indicated the general prevalence of thunder-storms, though they did not approach the metropolis within several miles: but the intense heat subsided. The wind continued during the greater part of June from the east or north-east, which rendered the mornings and evenings chill; but during the last two months it has been chiefly from the west and north-west.

During the months of June and July, the measles were extensively prevalent, but, as far as my observation went, neither more severe nor more fatal than usual. In some cases, and especially in children somewhat advanced in growth, as from 7 to 10 years of age, though the eruption was plentiful, the whole train of concomitant symptoms was particulary mild. In infants and young children, and more especially in those living in close and crowded situations, the pulmonary symptoms and fever were more severe, and cough, ear-ache, or inflammation of the eyes succeeded the termination of the disease. The only fatal instances which occurred under my observation, were in two very young children in the same family, who resided in a cellar, in a very close and crowded court. The elder of the two had been previously for some time in a sickly state, with symptoms of marasmus. Scarlet-fever has also been somewhat prevalent, and in a mild form, with

with but slight affection of the throat; and there has been some small-pox; but of the extent of the prevalence of the latter, the applications to the Dispensary afford no criterion, because the common cases are allowed to run their course, and assistance is only called for where something occurs to excite particular alarm. The prejudices against vaccination unfortunately do not appear to be diminishing: for in the family, in which the three cases in the list occurred, there were two older children who had been vaccinated, and who now remained in the midst of the variolous contagion with impunity, sleeping in the same bed with the infected; yet the parents had been led to question the security of vaccination, and even now were not easily impressed with the lesson, though one of the children lost an eye by the small-pox. Nor is this the only instance of the kind, which has occurred, in which a former confidence in vaccination has been withdrawn, without any actual evidence in disparagement of it, but even contrary to personal experience of its value; so much is the opinion of the uneducated under the influence of prejudice.

The derangements of the chylopoetic organs are, as usual, among the more prominent complaints of the season. Not only the cases of enterodynia, vomitus, diarrhœa, dysenteria, and cholera, but the majority of the instances of febris, vertigo, cephalæa, and asthenia, originate in the same source. Almost all the cases of fever were of the gastric or bilious class, and characterized by deranged excretions from the bowels, with pain in the epigastrium and abdomen, a much loaded tongue and depraved taste in the mouth, and complete loss of appetite, and by the absence of any tendency to delirium, or marked affection of the head. In several cases, the tongue has been highly red, with a disposition to chap, while the stomach has been very irritable, rejecting every thing but the mildest fluids in small quantity, and the bowels have been in a lax and scarcely less irritable condition; the countenance at the same time, with the exception of an occasional flush, remaining nearly natural in its expression, and the eyes without any tendency to suppression or dulness. In this condition the patient has continued, with very little alteration in the symptoms, for a fortnight or more; for the irritability of the whole alimentary canal has rendered the use of almost every species of ingesta, whether food or medicine, a source of stimulation. Small quantities of light liquid food, with a simple cretaceous draught, and blistering the epigastrium, have appeared to comprise all that was useful in the way of treatment: for even common saline medicines occasioned griping and purging, opiates and antimonials increased the febrile condition, or produced sickness, and every thing of a cordial quality was attended

by a similar effect. These complaints, however, have in general gradually terminated in convalescence. T. B.

London, August, 31, 1814.

Extract of a Letter from THOMAS SUTTON, *M. D. late Physician to the Forces, and consulting Physician to the Kent Dispensary.*

SIR,—I sometime ago read in your Journal a communication from Dr Harness, with a letter from Dr Burnett on the subject of the practice of blood-letting in a certain species of fever; and finding a book lately published by Dr Burnett on this subject, I was induced to obtain an early perusal of it. My curiosity was more particularly excited, on account of my having been, as I believe, the first who has in any sort of regular manner recommended this practice of late in Europe, as may be seen by my account of the disease in the pamphlet inclosed.* That blood-letting has been recommended in fevers by many in this quarter of the globe, and that it has been employed by some in this species of fever, cannot be doubted; but the practice has then been connected with many other remedies, and without defining the disease by any regular account of symptoms and dissections, so that it may be distinguished from other forms of fever. In reading Dr Burnett's account of this disorder, and the practice, I could not help observing several remarkable coincidences between his observations and mine, which I have taken the liberty of pointing out, by references to his work on the margin of my pamphlet, which must induce very strong reasons to infer, that the diseases we have each described, in the publications alluded to, are the same, and that modifications and aggravations of some of the symptoms may be referred to the difference of climate in which we practised. I am happy, too, to find that Dr Burnett has recommended a treatment, simple in itself, and obviously called for, from the concourse of symptoms, and the dissections, as well as by its success; and that he has also not failed to reject some remedies of doubtful efficacy, and has expressed his doubts about the propriety of trusting to several others, except in a very limited way; by which proceeding he will bring the reader to contemplate more seriously, and rely more firmly upon those which have proved of the most abundant advantage in his practice. It is easy to mention a number of remedies for the cure of fever, but to institute a course of inquiry, as to those that ought to be most relied upon, and to shew the inefficacy of others, at the same

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* A Practical Account of a Remittent Fever, frequently occurring among the troops in this climate. pp. 42. 8vo. London, 1806.

same time to describe the nature and prominent symptoms of the disease in which they are applicable, is neither so easy nor very common. Precision in description, and simplicity and aptness of remedies, are the chief things desirable in the art of medicine; although we frequently find patients, and even practitioners, much pleased with what they consider to be a variety, and store of resources, not considering that much variety of remedy, in the treatment of any one disease, unless similar in quality and effect, can only lead to the certain conclusion, that the disorder is of a nature not sufficiently understood, and is therefore treated without any definite views and intentions. As I believe you have not noticed my pamphlet in any of your preceding numbers, it may be possible that you will feel inclined to do so, on the present occasion, * as the practice there recommended was commenced by me, and embodied in a precise form many years ago, and becomes of additional interest to the public in proportion to its being more extensively employed.

In addition, I may be allowed to remark, that, although I do not accede to the doctrine that nothing more is necessary to excite the disease in question than marsh effluvia, I cannot avoid thinking, on this very delicate and obscure question, and in regard to the opposition of opinions that have been maintained, that the conclusion which is frequently made respecting disagreements in the common intercourse of life, that both parties may be right, would not be misapplied here; and that the discussions respecting the propagation of this fever by contagion, or otherwise, will at last be resolved into the probability that both parties have in a certain degree drawn just inferences. I have hitherto abstained from any discussion of this subject, but I may, at no very distant period, take the liberty of presenting you with my reasons for advancing such an opinion.

Report of the National Vaccine Establishment; dated 25th May 1814.

To the Right Hon. Lord Viscount Sidmouth, Principal Secretary of State for the Home Department, &c. &c. &c.

National Vaccine Establishment, Leicester Square, 25th May 1814.

My Lord,—The board of the National Vaccine Establishment has the honour to report to your Lordship, that the public confidence in the efficacy of vaccination continues to extend through every part of the world, and

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* We conceive that we take the most speedy and effectual method of complying with Dr Sutton's wish, by printing his letter, and referring to the publication itself.—*Editor.*

and the mortality from small-pox to decrease in the same proportion. During the year 1813, the number of persons vaccinated at the nine stations appointed under the authority of this Board, was 4274, and some addition to this number may be expected in the current year, from the late establishment of three new stations. The attention of the Board has been particularly directed to the distribution of vaccine lymph, and 25,394 charges of it have been furnished to the public. It has happened not unfrequently that the genuine matter has been lost in a whole district, and that the supply thus afforded has reproduced the infection, and checked the small-pox, which has been threatening to extend its ravages.

The principal station has also served as a school for the practice of vaccination, and, since its first establishment, nearly 500 surgeons have been instructed in the best modes of performing it, and have been enabled to introduce it in the most advantageous manner into the different parts of the British empire, through which they have been dispersed. In London, the deaths from small-pox have been 898, which is less than three-fourths of the mortality of the preceding year, though still somewhat greater than in 1811.

According to official communications from the medical colleges of Edinburgh, Glasgow, and Dublin, and from other numerous correspondents, it appears, that the benefits of vaccination have continued to be diffused throughout the civilized world. The governors of the Cape of Good Hope, and of Ceylon, by the humane regulations which they have adopted, have totally extinguished the small-pox in those settlements; and, by means of the salutary regulations which have been introduced in India, the frequency of this disease has been very much diminished throughout that Continent.

We have also great pleasure in stating, that the General Assembly of Jamaica has passed a law for the formation of an institution for the diffusion of the vaccine disease, which is to correspond with this Board; an example which, we trust, will be imitated in other parts of the West Indies.

Nor have the benefits of this institution been wholly confined to the British dominions. Sir Gore Ouseley, before his departure on his embassy to Persia, was furnished with an ample supply of the vaccine lymph, and with a variety of documents and instructions relating to the subject; and the Board has been highly gratified by a letter from that ambassador, stating that the sons and daughters of the Prince Royal of Persia, together with 1400 or 1500 other Persians, had already been vaccinated, and that the practice was extending through Tehran, the capital of that kingdom.

The failures of vaccination, which formerly occasioned so much alarm, are now become extremely rare, from the improved methods which have been adopted. Most of these failures appear to have arisen from the practice of vaccinating by a single puncture, and afterwards opening the vesicle, and taking a portion of the lymph for the purpose of propagating the infection. Many practitioners, reasoning from the analogy of the small-pox, conceived that such a practice could be productive of no inconvenience; but experience has shewn that it sometimes frustrates the expected benefit.

In

In one instance which had been thus treated, a fatal attack of the small-pox ensued. But in most of such cases, even when the small-pox actually occurred, the disease was so much mitigated as to be devoid of danger. These accidents may, however, be almost always prevented by exciting two or more vesicles, and by carefully observing that one of them should pass completely through all the stages of the vaccine process, unopened and undisturbed. Whenever the slightest irregularity has occurred, the patient ought to be revaccinated for his security.

The extreme infrequency of failures, when due precautions are employed, may be estimated by the success of an individual, who resides near Conway in North Wales, and who has informed the Board that he has vaccinated above 10,000 persons, without a single failure. Since the commencement of this establishment, only one failure has been suspected at any of the stations under its immediate direction; and even in that case the eruption dried up and disappeared more rapidly than in the regular small-pox. On the whole, there are ample grounds for believing, that, when proper attention is paid to the process, the failures from vaccination are not more frequent than from the most apparently perfect inoculation of the small-pox. Nor has the interval of sixteen years, which have elapsed since the publication of Dr Jenner's discovery, tended in any degree to justify the hypothetical objection which was raised by the original opponents of vaccination, "that its power of preventing the small-pox would probably last for six or seven years only."

But all the accumulated proofs of the utility of vaccination have not yet occasioned its universal adoption, nor have the fatal results of small-pox inoculation led to its being entirely relinquished. By far the most numerous, and all the eminent part of the medical profession, having abandoned this practice, some few of its members have only found the stronger motives for persisting in it, and for holding out to the poorer classes the fallacious pretence of performing the operation gratuitously; nor have arguments, example, or shame, prevailed on a certain class of persons to desist from a practice so pernicious, to which we cannot but attribute a great proportion of the mortality from small-pox, which still occurs in this metropolis, and which, in all probability, from the number of unregistered burials, exceeds the actual returns of the weekly bills by about one half. The small-pox has thus, by its continued existence in constant succession, become a much more destructive disease than the plague, which has occasionally visited this country; and it is to be lamented, that the regulations for the prevention of the promiscuous intercourse of the infected with society at large, which have at different times been so prudently adopted with respect to one of these diseases, cannot, in the existing state of the laws, be extended with equal facility to the other.

The whole of the expences incident to this establishment, for 1813, were defrayed by the vote of parliament which passed last year; and a grant to the same amount will probably be sufficient for the expences of the present year.

By order of the Board, J. Latham, *President.*

Jas. Hervey, *Regr.*

Copy of a Letter from the President of the National Vaccine Establishment, to Lord Sidmouth, dated 15th July 1814;—enclosing, Report on the State of Vaccination in Sweden.

N. V. E. Leicester Square, 15th July 1814.

My Lord, The Board of the National Vaccine Establishment have received the enclosed report 'On the State of Vaccination in Sweden,' since they had the honour of communicating to your Lordship their report of the state of vaccination during the year 1813; and they respectfully desire that it may be submitted to the honourable the House of Commons, in order that it may be printed, and subjoined as an appendix to their report. (Signed) J. Latham, President.

Jas. Hervey, M. D. Registrar.

To the Right Honourable Lord Viscount Sidmouth, &c. &c.

Report of the State of Vaccination in Sweden.

(Translated from the original in Swedish.)

On the fourteenth of January 1814, Mr Macmichael, an English gentleman, attended the Royal College of Health in Stockholm, and delivered to the College a copy of the Report of the National Vaccine Establishment in London, dated the 22d April 1813, and presented to Lord Sidmouth, Secretary of State for the Home Department; at the same time he requested, that a short account of the progress of vaccination in Sweden, and of the measures which had been adopted for its promotion, might be communicated to him, for the information of the British Parliament.

The Royal College had particular satisfaction in receiving Mr Macmichael, and undertook to comply with his request so much the more readily, as it had the pleasure of numbering among its honorary members, the respectable name of Dr Jenner, for whom it was reserved to demonstrate, by the most decisive experiments, the protective power of the cow-pox against the most terrible and destructive contagion of the small-pox; a pestilence, which, by means of this blessed discovery, must certainly be ultimately extirpated from the face of the earth.

It was to be expected, from the excellent arrangements which the Kings of Sweden had adopted for somewhat more than half a century, in every department of medical science, that the incomparable discovery of Doctor Jenner, like the inoculation for the small-pox at a former period,* should not only become an object of the most accurate investigation, but also, when approved by experience, be generally introduced and promoted by rewards and established regulations.

The

* It is remarkable, that the celebrated Dr David Schultzenheim, who was appointed as long ago as 1754, by the states of the Kingdom, to inquire into Sutton's and Dimsdale's mode of inoculating the small-pox in England, is now the President of the Royal College of Health, and has been the most instrumental, by means of his powerful influence, in promoting the most salutary measures for the introduction of vaccination.

The medical practitioners of Sweden, who had already been informed from the time of Dr Jenner's first discovery, by means of a constant correspondence with the learned in other countries, of the expectations which were entertained of the success of experiments made with the cow-pox, had great pleasure in learning that one of their colleagues, Dr Gahn, a member of the Royal College, had, towards the end of 1799, procured some vaccine matter, and obtained the most satisfactory result from inoculating with it. Another Swedish physician, now Professor of Medicine, Dr Rosensköld, printed, in 1801, a small pamphlet, entitled, 'To the Public on the Cow-Pox;' and performed vaccination with success in several parishes in Skäne. About the same time the undersigned also published a more detailed account, with coloured figures, under the title, 'The Small-Pox may be extirpated;' and this essay was distributed to all the churches in the kingdom.

The government, already attentive to the inestimable advantage which the inoculation of the cow-pox seemed to promise, directed the College to examine Dr Jenner's discovery with the greatest accuracy, for which the proper means were immediately afforded; and the College was ordered, after collecting the results, to present its report to the King.

This report, which fully confirmed the excellence of the Jennerian discovery, occasioned the salutary law which was first enacted in 1803, by which vaccination was established throughout the kingdom; and the College was commanded to promote its adoption by all possible means. The King was pleased to appropriate 900 dollars, spec. banco, to be divided into premiums, which were to be distributed among such medical men as could exhibit the greatest number of vaccinated persons.

A particular regulation was made for the metropolis, imposing a fine of three dollars on any one who should fail to announce to the medical officer of the district, the appearance of the contagion of the small-pox; and, in every such case, the person infected was to be carried to the small-pox hospital, where every measure was adopted for his being properly nursed; and the same precautions have been continued to the present time.

It was long a question, Whether new-born children could be vaccinated with success, and whether the matter taken from them might be employed with as much security as if taken from adults?

This doubt has been altogether removed, and in the General Lying-in Hospital, all the children are now vaccinated within nine days from the time of their birth; so that, by means of this progressive vaccination, fresh matter remains constantly in existence.

The want of a sufficient supply of vaccine matter for the extensive provinces of the kingdom, was long an obstacle to the universality of vaccination of Sweden. This obstacle no longer exists; since the Royal College of Health, in consequence of the humble representations which it made to the king, obtained the adoption of a very effectual measure for this purpose, in the appointment of a particular establishment for the general regulation of vaccination throughout the kingdom, which took place in the year 1812.

This establishment consists of a director, and several inspectors of the stations for vaccination in the provinces. The director is a member of the Royal College of Health, whom the king has graciously commanded to

receive

receive and examine all reports, to answer all inquiries, to conduct the distribution of vaccine matter, which is delivered, free of postage, to all persons who apply for it; and lastly, to report to the College every thing relating to vaccination that requires further regulation, and to propose to it, as proper persons to receive rewards, all those who appear to be the most deserving. He has also the immediate inspection of all the medical men, who are appointed to conduct the business of the stations established in almost every province; the progressive vaccination performed at these stations being calculated to maintain a constant supply of fresh matter, which is also distributed, free of postage, to those who require it; and their proceedings being registered in proper catalogues and journals.

In Stockholm, three several stations of this kind have been appointed, whence fresh matter may always be procured with certainty, if it happen to be wanting in any particular province.

The archbishop, bishops, and the whole of the clergy throughout the kingdom, having, from the time of the happy discovery of vaccination, embraced it with the most distinguished zeal; and many of this respectable body having not only employed the most effectual means for the removal of vulgar prejudices against it, but having even actually practised vaccination themselves; the king, assured of the continued exertions of the clergy in the same cause, was pleased to direct, that every minister should superintend the progress of vaccination within his parish; and should be empowered to call to his assistance one or more inspectors of vaccination, according to circumstances, for the purpose of causing all children to be properly vaccinated within the first year after their birth, and keeping proper documents of the performance of the operation. In each parish or district there must be an accredited vaccinator, whose duty is to perform vaccination, and to give a report of his proceedings to the Royal College of Health.

The College has also published, by the King's command, a book of instructions for vaccinators and inspectors of vaccination, which has been distributed gratis to all the churches in the kingdom. This treatise, adapted to the use of the public, affords an accurate knowledge of the true and false cow-pox; of the varieties which most frequently occur in it; and of the cutaneous diseases, which occur so often in Sweden, very nearly resembling the small-pox.

For the more effectual encouragement of the practice of vaccination, the King has been graciously pleased to appoint rewards of two different kinds, pecuniary premiums and honorary medals. The latter are distributed commonly in silver, but sometimes in gold, to those who have particularly distinguished themselves. In all cases, those who have deserved rewards, are humbly pointed out to the King, by the College of Health; and his Majesty has reserved to himself the right of assigning the proportions in which those rewards shall be distributed. It is also in the King's name, and with a certain degree of publicity, that these marks of his approbation are bestowed.

For the honour of the medical profession in Sweden, it must not be forgotten, that although inoculation for the small-pox was one of the most lucrative branches of their private practice, and has been entirely superseded by the simple process of vaccination, no one individual of the profession has

has raised any obstacles against the cow-pox; but every one has contributed to its advancement, by giving advice, information, and assistance, to the utmost of his ability. No single publication has appeared to call in question its high importance, * and its superiority to variolous inoculation; which has been entirely discontinued ever since the year 1802, rather by a tacit and universal consent, than in consequence of any royal prohibition.

It may, therefore, be asserted, that the small-pox, that equally disgusting and destructive pestilence, which for many ages continued annually to send out of the world an immense number of young children, is now, through the influence of Dr Jenner's inestimable discovery, so perfectly extirpated in Sweden, that it never can become epidemic, even if at any time, notwithstanding all the orders and all vigilance employed for its exclusion, the infection should make its appearance. Such, in the last twelve years, had been the effect of the King's wise and humane attention, of the unanimity and disinterestedness of the medical profession, of the patriotic zeal of the clergy, of the good examples so promptly exhibited by the upper classes, and of the progress of information and civilization in the lower.

The undersigned, who has drawn up this short account at the request of the Royal College of Health, has also the honour of sending with it, in the name of the College, a copy of the book of instructions, and an impression in silver of the honorary medal, which was struck by the King's command, under the direction of the College, and which is distributed in the King's name, for the promotion of vaccination.

Fr. Hedin, M. D.

First Physician to the King, Medical Counsellor, &c.

Stockholm, 10th February, 1814.

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* The answer which the undersigned returned the 1st November 1801, to a letter addressed to him, by the vaccine committee of the Society of Medicine at Paris, and which is inserted in the second report of that committee, cannot justly be considered as a publication of this kind. It was not quite three months after this time, that having acquired perfect confidence from inoculating a cow with the cow-pox, and transferring the operation to the human subject, he published the before-mentioned essay, entitled, "The small-pox may be extirpated."

Cow Pock Institution, No. 62, Sackville-Street, Dublin.

An Abstract from the Register of Inoculations and Distribution of Infections.

	Patients Inoculated.	Packets issued to Practitioners in general.	Packets to Army Surgeons.
1804	578	776	236
1805	1,032	1,124	178
1806	1,356	1,340	220
1807	2,156	1,790	320
1808	3,002	2,285	333
1809	3,941	2,540	244
1810	4,084	3,249	284
1811	4,157	3,838	368
1812	5,162	3,901	402
1813	4,968	4,465	314
Totals.	30,436	25,308	2,899

In submitting to the Public a Report for 1813, the Directors of the Cow-Pox Institution have the pleasure to record a remarkable diminution in the frequency of small-pox throughout this city. A death from that disease, even among the poor, has been comparatively a rare occurrence, and as the inoculation for small-pox is nearly discontinued, there is strong reason to hope that Dublin will soon afford a lasting proof of the beneficial effects of vaccination. During ten years which have elapsed since the establishment of this Institution, the Directors have felt it their duty to pay particular attention to the progress of the new inoculation, to observe its effects on the constitution, and to ascertain the extent of its preventive powers; and they have great satisfaction in stating, that no circumstance has occurred to lessen their confidence in the practice, but that each succeeding year has afforded additional evidence of its efficacy and safety.

Since last report, no case has occurred of small-pox following vaccination, either at the Institution, nor in the private practice of the gentlemen connected with it.

The establishment of Dispensaries throughout Ireland, has had a great effect in extending the benefits of vaccination, but it is to be lamented, that the lower orders cannot be convinced of the necessity of having their children examined at the proper periods after inoculation, from want of attention to which circumstance, many of those inoculated must remain in a doubtful state, and liable to small-pox. To obviate so great an evil, it is earnestly recommended to those engaged in vaccinating the poor, to exact such a deposit as shall ensure their regular return. Such a regulation might lessen the number of applicants, but it would ensure an increase in the number of satisfactory inoculations.

The Directors are happy to find, that, among the country practitioners, there

there is no relaxation in their zeal for promoting the practice, but they are sorry to add, that the philanthropic exertions of these gentlemen to exterminate small-pox, by the general substitution of cow-pock, have, on many occasions, been counteracted by Itinerants, who traverse the country, particularly during the Spring and Autumn, inoculating with the small-pox the poor of the villages through which they pass. The introduction of this disease into places, whence it had been banished by vaccination, has, on many occasions, been traced to this improper practice. It is to be hoped that the Legislature may interfere to protect the community, by preventing these impostors from disseminating a complaint so injurious to population. Until some strong measures be adopted, the benefits of vaccination, particularly in the country parts of Ireland, must be very partial.

In April last, a child, seven months old, was attacked on the 12th day of cow-pock, with convulsions, followed by an erysipelatous fever, which proved fatal in ten days. The eruption commenced around the vesicle, spreading over the whole of the inoculated arm and hand. In a few days it gradually extended over the trunk, face, and lower extremities, leaving the parts it had first affected. The symptomatic fever was considerable, and the child appeared to suffer much from pain. Shortly before death the legs and arms became œdematous.—It should be remarked, that, at the time of this child's illness, erysipelas was frequent among the children in the neighbourhood.

Signed by Order,

January 6, 1814. SAM. B. LABATT, *Secretary.*

On the 24th of June, the Senatus Academicus of the University of Edinburgh conferred the degree of Doctor in Medicine on the following gentlemen, after having gone through the appointed examinations, and publicly defended their respective inaugural dissertations:

Of SCOTLAND.

William Moncrieff,	*De Peste.*
James Low Warren,	*De Hydrope Anasarca.*
William Turnbull,	*De Pneumonia.*
William Birrell,	*De Hydrope Anasarca.*
Robert E. Grant,	*De Circuitu Sanguinis in Fœtu.*
Claud Francis du Fresne,	*De Hydrargyro.*
James Thomson,	*De Viribus Systematis Naturæ Medicatricibus (vulgo) dictis.*
Robert Knox,	*Experimenta de Viribus Stimulantium et Narcoticorum in Corpore sano continens.*
Robert Lee,	*De Rheumatismo.*
Nathaniel Grant,	*De Ischuria.*
James Sibbald,	*De Asphyxia.*
William Balfour,	*De Angina Pectoris.*
Robert Renton,	*De Digestione.*
Andrew Fyfe, jun.	*De Copia Acidi Carbonici e Pulmonibus inter Respirandum evoluti.*

James

James Colquhoun,	*De Enteritide Acuta.*
William Whiteside,	*De Rubeola.*
Thomas Henderson,	*De Cynanche Tracheali.*
Patrick Robertson,	*De Dyspepsia.*
William Gordon Watson,	*De Sanguine.*
	From ENGLAND.
James Rawlins Johnson,	*De Hirudine.*
Henry Ronalds,	*De Ventriculi Fabrica Vitiata.*
Edward Carbutt,	*De Systematis Nervosi Physiologia.*
Richard Bent,	*De Podraga.*
Edw. Welchman Wright,	*De Podraga.*
John Lane Green,	*De Impotentia et Sterilitate.*
George Henry Hicks,	*De Dyspepsia.*
John Davy,	*De Sanguine.*
Jonas Malden,	*De Augmentis Scientiæ Medicæ.*
Richard Burgess,	*De Mulierum Infœcunditate.*
John Jeremiah Bigsby,	*De Vi Arsenici Exitiosa.*
Frederick Love Hammick,	*De Ulcere Maligno.*
James A. Gordon,	*De Arsenico.*
Henry Parker,	*De Morbis Balneo Frigido Medendis.*
	From IRELAND.
Patrick Macmahon,	*De Bile.*
John Boston,	*De Opii usu in Morbis quibusdam.*
Gerard Barry,	*De Colica Pictonum.*
David Lithgow,	*De Sanguinis Detractione.*
John Mollan,	*De Hæmorrhoide.*
Arthur Jacob,	*De Aneurismate.*
William Ryan,	*De Ischuria Vesicali.*
Gilbert George Newsom,	*De Scrophula.*
Hugh Cuningham,	*De Asthmate.*
Alexander Skipton,	*De Carcinomate.*
Samuel Cherry Unthank,	*De Fœtus Nutrimento.*
William Lloyd,	*De Morbis Genu Humani Symptomaticis.*
Charles Bingham Hill,	*De Dyspepsia.*
Michael M'Mahon,	*De Typho.*
James Rutherford,	*De Febre Flava.*
George Ward,	*De Hepatitide.*
Michael M'Guirk,	*De Febribus Intermittentibus.*
James L. Drummond,	*De Oculi Anatomia Comparativa.*
	From WALES.
Walter D. Jones,	*De Phthisi Pulmonali.*
	From RUSSIA.
Charles Simpson,	*De Hepatitide.*
	From PORTUGAL.
Joseph C. Vaz,	*De Differentiis in Secunda Fabrica, Phænominisque ab hisce Pendentibus.*
Joseph E. Gomes,	*De Hepatitide.*

From

From the East Indies.

James Kellie, *De Apoplexia.*

From Funchal (Madeira.)

James Ludovick Pestana, *De Cœlo Calido Temperato Animalia Juvante.*

From Fatal.

Emmanuel da S. Rodrigues, *De Febre Flava.*

From Jamaica.

Thomas M'Kenzie, *De Febre Flava.*

Joseph Forsyth, *De Hepatitide.*

From Antigua.

Anthony Musgrave, *De Præcipuis Morbis Indiæ Occidentalis.*

From Nevis.

Alexander Hanley, *De Memoria.*

And, on the 12th of September, the same honour was conferred on the following Gentlemen:—

Of Scotland.

Robert Nimmo, *De Gastritide.*

Robert Miln, *De Apoplexia Sanguinea.*

William Ramsay, *De Nexu inter Animum Corpusque.*

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From the EAST INDIES.

William P. Hodge, *De Rubeola.*
Benjamin Bishop, *De Frictione.*

Medical Lectures in the University of EDINBURGH.

Dietetics, Materia Medica, and Pharmacy	Wednesday, Oct. 26.	8	Dr James Home
Practice of Physic		9	Dr Gregory.
Chemistry & Chemical Pharmacy		10	Dr Hope.
Theory of Physic		11	Dr Duncan, sen.
Anatomy, Pathology and Surgery		1	Drs Monro.
Theory & Practice of Midwifery	Tu. Nov. 8. 3 o'clock.		Dr James Hamilton.
Military Surgery	Saturday, Nov. 12. 2 o'clock.		Dr Thomson.
Clinical Medicine	Tues. Nov. 8. 4. P. M.		Dr Home. Dr Duncan, sen.
Clinical Surgery	Monday, Oct. 31. 5. P. M.		Mr Russell.

Practical Anatomy under the superintendence of Dr Monro, jun.

During the Summer-Session, Lectures will be given on the following Branches of Medical Education:—

Botany, by Dr Rutherford.
Midwifery, by Dr Hamilton.
Clinical Lectures on Medicine, by Dr Duncan, sen.
Clinical Lectures on Surgery, by Mr Russell.
Medical Jurisprudence, by Dr Duncan, jun.

The Royal Medical Society of Edinburgh propose, as the subject of the Prize-Essay for the years 1814 and 1815,

"The comparative specific caloric of Venous and Arterial Blood."

A set of Books, or a Medal of five guineas value, shall be given to the author of the best dissertation on an experimental investigation of the subject proposed; for which all the members, honorary, extraordinary, and ordinary, are alone invited as candidates.

The dissertations are to be written in English, French, or Latin, and to be delivered to the Secretary on or before the 1st of December of the succeeding year to that in which the subjects are proposed, and the adjudication of the Prize shall take place in the last week of February following.

To

To each dissertation shall be prefixed a Motto, and this Motto is to be written on the outside of a sealed packet, containing the name of the author. No dissertations will be received with the author's name affixed; and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

The following Works have been recently published:

Letters addressed to his Royal Highness the Duke of Kent, on Consumption; containing Remarks on the Efficacy of Equable and Artificial Temperature in the Treatment of that Disease, &c. By Thomas Sutton, M. D. of the Royal College of Physicians, late Physician to the Forces, and Consulting Physician to the Kent Dispensary; and author of Tracts on Delirium Tremens, Gout, &c. Price 2s. 6d.

Practical Observations on Insanity; in which some Suggestions are offered towards an improved Mode of Treating Diseases of the Mind; and some Rules proposed which may lead to a more humane and successful Method of Cure; with Remarks on Medical Jurisprudence, as it relates to Diseased Intellect. By Joseph Mason Cox, M. D. 8vo. Price 6s. boards.

An Essay on Bronchitis; with a Supplement containing Remarks on Simple Pulmonary Abscess, &c. &c. By Charles Badham, M. D. Second Edition, corrected and enlarged, boards, 5s. 6d. 12mo.

Elements of Medical Jurisprudence, or a succinct and compendious description of such tokens in the Human Body as are requisite to determine the judgment of a Coroner, and of Courts of Law in cases of Divorce, Rape, Murder, &c. To which are added, Directions for preserving the Public Health.—By Samuel Farr, M. D. Second Edition, 12mo. boards, 5s. 6d.

A Treatise on the Antiphthisical Properties of the Gratiola Officinalis, or Hedge Hyssop, with Directions for its use in Pulmonary Consumptions and Asthmatic Complaints. By Richard Reece, M. D. Member of the Royal College of Surgeons, Author of the Medical Guide, &c. &c. Price 2s.

Commentaries on the Treatment of the Venereal Disease, particularly in its exasperated state, containing Advice relative to the Use of Mercury, so as to insure its successful effect; with an Appendix, recommending an Improvement in the Treatment of Strictures. By Edward Geoghegan, Member of the Royal College of Surgeons in Ireland, &c. &c. 8vo. Price 6s. 6d. boards.

A treatise on the Gout, containing the opinions of the most celebrated ancient and modern Physicians on that disease, with observations on the Eau Medicinale By John Ring, Member of the Royal College of Surgeons in London. 8vo. 16s.

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Practical Observations on the best mode of curing Strictures in the Urethra, with Remarks on the inefficacy and ill effects of Caustic applications. By William Wadd, Member of the Royal College of Surgeons, London. New edition. 8vo. 4s.

Facts and Observations deduced from long and extensive practice in Liver Complaints and Bilious Disorders in general, and on such derangement of these Organs as influence the Biliary Secretion, with some new and practical observations on the various appearances of this important Secretion, connected by an appropriate and successful mode of Treatment, and the whole illustrated and confirmed by a numerous list of cases. By John Faithorn, formerly Surgeon in the East India Company's service. 8vo. 5s.

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St George's Hospital, *and Great George Street, Hanover Square.*—In the first week of October will commence a course of Lectures on the Practice of Physic, Therapeutics, and Chemistry, at No. 9, Great George Street, Hanover Square. The medical lectures at eight, and the chemical at a quarter after nine o'clock; by George Pearson, M. D. F. R. S. &c. A register is kept of Dr Pearson's cases in St George's Hospital, and an account is given of them every Saturday morning, at a clinical lecture, at nine o'clock.

Dr Clough, 68, Berner's Street, Physician Accoucheur to the St Mary-la-bonne General Dispensary, &c.; commences his Autumnal Course of Lectures on the Science and Practice of Midwifery, including the Diseases of Infants, on Monday morning, the 10th of October, at half-past ten, and his evening course, at seven. Particulars known as above, and of the Medical

dical booksellers. House-pupils may be accommodated, and the same extensive opportunities of attending cases for practical improvement, will be afforded the students, when qualified, as usual.

Dr MERRIMAN will deliver his usual Courses of Lectures on the Theory and Practice of Midwifery, and the Diseases of Women and Children, at the Middlesex Hospital, during the Autumnal and Winter months. The Introductory Lecture will be read early in October.

In the Press.

A Synopsis of the Various Kinds of Difficult Parturition, including Practical Remarks on the Management of Labours, and forming a Manual of Midwifery. By SAMUEL MERRIMAN, M. D. Lecturer on Midwifery at the Middlesex Hospital, &c. &c. 2d Edition, much enlarged.

Dr BALFOUR desires us to intimate, that, since his paper on the Reunion of Separated Parts was printed in the Journal, he has reprinted it in the form of a pamphlet.

Communications have been received from Mr BRYCE, Mr PEAKE, Mr BALLINGALL, and Dr KNOX.

ERRATA.

For Dr Macmillan, *read* Dr Macmullin.

Page 31, line 27, *for* typhoid congestion, *read* topical congestion.

34,——31, *for* ætheris nitrosi tinct. opii, ℥ ā ℥i, *read* ætheris nitrosi tinct. opii, ℥ ā ʒi.

37,——14, *for* applies only to intermittents, *read*, applies to intermittents.

*** Communications may be addressed to the Editors, to the care of Messrs CONSTABLE & Co. Edinburgh; Messrs LONGMAN, HURST, REES, ORME, & BROWN, London; and GILBERT & HODGES, Dublin.

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